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FACULTEIT PSYCHOLOGIE EN
PEDAGOGISCHE WETENSCHAPPEN

BREIN & COGNITIE

Launching awareness and chasing consciousness

Perceptual organization and continuous flash suppression

Pieter Moors

Proefschrift aangeboden tot het verkrijgen
van de graad van Doctor in de Psychologie

Promotor: Prof. Dr. Johan Wagemans
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Samenvatting

Het mysterie van hoe wij ons bewust worden van de wereld rondom ons, vormt voor filosofen, psychologen, en neurowetenschappers reeds decennia- zo niet eeuwenlang een bron van fascinatie. Onderzoek naar dit onderwerp heeft onder meer aangetoond dat er geen één-één relatie is tussen de fysieke input die op ons netvlies valt en de fenomenologie van onze visuele waarneming. Zo is het in specifieke gevallen bijvoorbeeld mogelijk om een visuele stimulus aan iemand te tonen zonder dat hij/zij deze effectief waarneemt. Het voorliggend doctoraatsonderzoek had als doel een beter begrip te krijgen over de representatie van een onzichtbare stimulus. Specifieker gesteld was onze overkoepelende onderzoeksvraag de volgende: *"Tot op welke hoogte vindt perceptuele organisatie nog plaats in de visuele verwerking van stimuli die perceptueel onzichtbaar gemaakt zijn door middel van 'continuous flash suppression'?"*

In het kader van dit doctoraat hebben we gebruik gemaakt van de methode van "continuous flash suppression (CFS)" om de zichtbaarheid van een stimulus te manipuleren. Het doctoraatsonderzoek is opgesplitst in twee verschillende delen. Het eerste deel focust voornamelijk op de onderliggende mechanismen van perceptuele onderdrukking die ten grondslag liggen aan CFS. In het tweede deel verschuift de focus voornamelijk naar de representatie van de onzichtbare stimulus.

In de eerste twee hoofdstukken worden twee studies beschreven over de factoren van doeltreffende perceptuele onderdrukking door middel van CFS enerzijds en de temporele eigenschappen van de tijdsduur van perceptuele suppressie anderzijds. In Hoofdstuk 2 tonen we aan dat de kenmerkoverlap tussen onzichtbare stimulus en CFS mask een belangrijker factor is voor doeltreffende onderdrukking dan de dynamische eigenschappen van het CFS mask. In Hoofdstuk 3 observeren we verder dat de tijdsduren van perceptuele suppressie niet onafhankelijk zijn van elkaar, maar dat ze subtiele sequentiële afhankelijkheden vertonen, een integraal kenmerk van perceptuele multistabiliteit. Op basis van de resultaten van deze twee studies concluderen we dat de mechanismes waarop CFS berust, gelijkaardig zijn aan die van binoculaire rivaliteit, en dat CFS meer dan waarschijnlijk een sterkere vorm van binoculaire rivaliteit is.

De volgende zeven hoofdstukken, die samen het tweede deel van deze thesis vormen, omvatten zeven studies die dieper ingaan op de representatie van de onzichtbare stimulus. Hoofdstuk 4 bevat een studie waarin we nagingen of het vlak geïnduceerd door de zogenaamde Kanizsa stimulus nog steeds gepresenteerd wordt tijdens perceptuele onderdrukking van deze stimulus. In Hoofdstuk 5 gingen we na of een hoorbaar geluid een invloed had op de verwerking van een onzichtbare stimulus. Hoofstukken 6 tot 8 bevatten respectievelijk studies waarin we aantonen dat causale gebeurtenissen sneller zichtbaar worden, biologische beweging geen inversie-effect vertoont, en gezichten in gekende configuraties sneller in het bewustzijn treden. Tot slot, in Hoofdstukken 9 en 10, bestuderen we of betekenisvolle aspecten van onzichtbare stimuli verwerkt worden. In Hoofdstuk 9 slagen we er niet in om een voorheen geobserveerd scene-congruentie effect te repliceren. In Hoofdstuk 10 tonen we vervolgens aan dat woorden niet verwerkt worden wanneer ze onzichtbaar zijn, door noch een woordfrequentie-effect, noch een effect van woordtype (woord vs. pseudo-woord) te observeren.

Samenvattend kunnen we stellen dat de resultaten uit het tweede deel van deze thesis duiden op een beperkte visuele verwerking van visuele stimuli die onzichtbaar gemaakt zijn door middel van CFS. De implicaties van deze resultaten worden uitvoerig besproken in Hoofdstuk 11.

Summary

The mystery of conscious visual experience has intrigued many philosophers, psychologists, and neuroscientists for decades, if not centuries. One of the insights research on this topic has yielded is that there is no one-to-one correspondence between physical visual input and our corresponding perceptual experience. Indeed, in some specific situations visual input can be presented to the observer, while remaining invisible. The ultimate goal of this dissertation was to achieve a better understanding of the representational nature of a perceptually suppressed visual stimulus. More specifically, the overarching research question was: "*To what extent does perceptual organization take place in the visual system for stimuli rendered invisible through continuous flash suppression?*".

Indeed, throughout this PhD we have relied on a paradigm called "continuous flash suppression (CFS)" to manipulate the contents of visual awareness. The research presented in this dissertation is divided into two main parts, the first part being oriented to more theoretical aspects regarding the mechanisms of suppression through CFS while the second part focuses on the representation of the suppressed stimulus.

The first two chapters present two studies on the determinants of effective suppression through CFS and the temporal characteristics of the time series of suppression durations. In Chapter 2, we show that feature overlap between the mask and suppressed stimulus is more important to enable effective suppression than the dynamic nature of the CFS mask. In Chapter 3, we report that suppression durations are not independent, yet show small, but significant sequential correlations, implying that CFS elicits a hallmark characteristic of other multistable phenomena, perceptual memory. Based on the results of these two studies, we conclude that CFS relies on mechanisms similar to binocular rivalry, and that it might constitute a stronger form of binocular rivalry.

In the following seven chapters, which comprise the second part of the dissertation, we present seven studies that tap into the representation of the perceptually suppression stimulus. In Chapter 4, we consider whether the surface induced by the classic Kanizsa stimulus is represented during suppression. In Chapter 5, we ask whether a supraliminal auditory stimulus can influence suppression strength of a perceptually suppressed looming stimulus. In Chapters 6 to 8, we show that launching events (an exemplary stimulus of causality perception) enter awareness faster than pass or pseudo-launch events, biological motion stimuli do not yield an inversion effect, and that faces presented in familiar configurations break suppression faster, respectively. Finally, Chapters 9 and 10 consider complex, high-level processing under CFS. In Chapter 9, we fail to replicate a previously observed result in which incongruent scenes broke suppression faster compared to congruent ones. In Chapter 10, we ask whether words are processed during suppression, but observe neither a word frequency nor a word type (word vs. pseudo-word) effect.

In sum, the results presented in the second part of this dissertation show very limited visual processing during CFS, the implications of which are discussed in Chapter 11.

Woorden van Dank

Een doctoraat is het resultaat van een collectieve inspanning, die hoofdzakelijk aan één enkele persoon geattribueerd wordt. Dit doctoraat vormt een Gestalt. Het geheel is meer en anders dan de som van de delen die eraan bijgedragen hebben. Mijn dank gaat uit naar de volgende "delen" die al dan niet bewust aan de totstandkoming van het geheel bijdroegen.

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Chapter 1.
General Introduction

I mean, what exact buttons do I have to hit?

Anthony Holland, 2011

Every time I wake up from a dark, seemingly dreamless night, something extraordinary happens: I open my eyes, and I *see* (Koenderink, 2012). When I sip from my coffee cup and I look outside my kitchen window, I witness something equally remarkable: "I see a house, trees, sky" (Wertheimer, 1923). I never see brightnesses, spatial frequencies, or orientations. This is fundamental. My visual experience of the world is organized and structured into distinct objects ordered in depth (Wagemans et al., 2012). It seems immediate and effortless. How is such rich conscious experience generated? Can we understand our phenomenology of the visual environment in terms of a complex transformation of the photons hitting the retina to visual awareness, involving several steps in a dynamic, hierarchically organized system entailing multiple feed-forward and feedback loops (Palmer, 1999)? Unfortunately, or rather, obviously, I will not be able to provide an answer to that question.

The mystery of visual awareness, or consciousness in general, has baffled many scientists for decades, if not centuries. The complexity of the problem has generated a rich and diverse literature that approaches consciousness from various angles. Philosophers, psychologists, and neuroscientists alike have provided many extensive, sometimes conflicting views, which has more than once yielded intense discussions on various topics (Cohen, Cavanagh, Chun, & Nakayama, 2012; Cohen & Dennett, 2011; Fahrenfort & Lamme, 2012; Tsuchiya, Block, & Koch, 2012). The inquiries range from formulating theories and models of consciousness (i.e., what constitutes the necessary and sufficient conditions for consciousness to "arise") (Baars, 1993; Cleeremans, 2008; Dennett, 1993; Jackendoff, 1987; Lau & Rosenthal, 2011; Oizumi, Albantakis, & Tononi, 2014; O'Regan & Noë, 2001) to studying states or levels of consciousness (e.g., what is the difference between an individual in a vegetative state versus one who is labelled with having a full conscious experience? is there some residual – or even full-blown? – consciousness left in the individual in the vegetative state?) (Owen et al., 2006). Can we pinpoint the neural correlates of consciousness, and if so, how should we interpret these (Crick & Koch, 1998, 2003; Dehaene & Changeux, 2011)? How is consciousness different from processes such as attention and working memory (Koch & Tsuchiya, 2007; V. A. F. Lamme, 2003; Soto, Mäntylä, & Silvanto, 2011; Soto & Silvanto, 2014; Stein, Kaiser, & Hesselmann, 2016)? Does it even make sense to dissociate these concepts, or

are these merely fruitless exercises in taxonomy in which scientists so often indulge themselves?

Although these are all interesting and relevant questions, this dissertation does not consider *what* consciousness is or *how* consciousness arises. This is taken for granted and we assume that some states of the visual system will yield conscious experience of a visual stimulus whereas others do not. Rather, we focus on the following general topic: *"which types of stimulus processing can bypass visual awareness, and henceforth do not necessarily rely on a conscious experience of the stimulus?"*

In particular, the relationship between perceptual organization and visual awareness is at the core of this dissertation. The apparent ease with which perceptual organization "happens" in our visual conscious experience has spurred the question whether we need to be conscious of the visual input at all for it to be (perceptually) organized (Alais & Blake, 2015; Schwarzkopf & Rees, 2015). Embarking on such an endeavor necessarily entails the choice of a proper paradigm to present observers visual stimuli they cannot see (or rather, fail to report). Let us consider the types of psychophysical magic that are at our disposal (Kim & Blake, 2005).

Rendering a stimulus invisible

A central issue in studying how invisible stimuli are processed is how to render a visual stimulus invisible in the first place (Breitmeyer, 2015; Kim & Blake, 2005). Breitmeyer (2015) recently reviewed all available paradigms, arriving at a list of 24 (sometimes subtly) different methods to render a stimulus invisible. The choice of a suitable paradigm is by no means a trivial one, and despite their upsides, each also comes with its downsides. A first possibility is to present stimuli at such an impoverished intensity, such that they can no longer be discriminated from the background. This method is not frequently used in consciousness research, because degrading stimulus intensity can have the unintended side effect that the stimulus is not processed anymore at all. Thus, a desirable property of a blinding paradigm is that it allows one to present stimuli at intensity levels such that they are detectable when presented independently.

An attractive set of paradigms that fulfills this criterion are visual masking paradigms (forward, backward, sandwich, metacontrast masking) (Breitmeyer & Ogmen, 2006). Here, a

stimulus is presented for a very short time (e.g., 16 or 32 milliseconds) and is either preceded or followed (or both) by a masking stimulus. Depending on the asynchrony between the target and masking stimulus, this renders the target stimulus fully invisible. The downside, however, is that stimuli can only be presented for a limited amount of time. Ideally, one would like to present a stimulus for more than some tens of milliseconds to allow for sufficient processing of the stimulus. Two different kinds of paradigms provide a potential solution to this: attention-based paradigms (Mack & Rock, 1998) and those involving dichoptic stimulus presentation (Alais & Blake, 2005). We will limit our discussion to the latter, because the former inevitably involves a manipulation of the attentional state of the observer, which changes the focus of the question in the direction of "can process X occur without attention?" Although blinding paradigms based on attentional manipulations also involve a manipulation of the awareness of the stimulus, the question whether awareness is necessary for processing stimulus property X becomes necessarily confounded with whether attention is necessary. This leads us to a last class of paradigms, in which one stimulus is presented to one eye while a different stimulus is presented to the other eye, at corresponding retinal locations. Although this situation occurs all the time in daily life due to retinal disparity (Arnold, 2011), a remarkable thing occurs when the mismatch between the input to both eyes is sufficiently large. Despite constant retinal input, our phenomenal experience of the input fluctuates in a seemingly stochastic manner between (mixtures of) the stimuli presented to both eyes, a phenomenon referred to as binocular rivalry (Blake & Logothetis, 2002; Blake, 1989; Kim & Blake, 2005; Tong, Meng, & Blake, 2006). Here, the advantage is that periods of dominance and suppression can last for several seconds, allowing one to render a stimulus invisible for a longer time period. A downside to this paradigm is that it is difficult to control how long a stimulus remains suppressed, but also which stimulus will be suppressed at onset. To control for initial dominance, flash suppression provides a solution (Brascamp, Knapen, Kanai, van Ee, & van den Berg, 2007; van Ee, 2011; Wolfe, 1984). In flash suppression, the to be suppressed stimulus is first presented for a second or so, before presenting the other stimulus to the other eye. This allows the visual system to adapt to this stimulus first, and reliably renders it invisible (suppressed) when the other stimulus is presented to the other eye. However, this is not a desirable situation if it is particularly important that observers are never aware of the

invisible stimulus. A solution to all of these problems – short stimulus presentation and reliable suppression at trial onset – is offered by a paradigm that has been introduced about 10 years ago, and on which we will rely throughout this dissertation.

Continuous flash suppression – a free lunch?

In this dissertation a paradigm called continuous flash suppression (CFS) is used to manipulate the contents of visual awareness (Tsuchiya & Koch, 2005; Tsuchiya, Koch, Gilroy, & Blake, 2006). CFS involves a situation of dichoptic stimulation in which a continuously changing mask is presented to one eye, while (most of the time) a static stimulus is presented to the other eye (see Figure 1.1 for an example). In most, if not all, cases this dynamic mask gains initial dominance over the other stimulus. Furthermore, in comparison with binocular rivalry, the CFS mask more strongly and reliably suppresses the other stimulus (Tsuchiya et al., 2006; E. Yang & Blake, 2012). These two attributes, initial dominance and effective suppression, have made CFS an attractive paradigm for consciousness research.

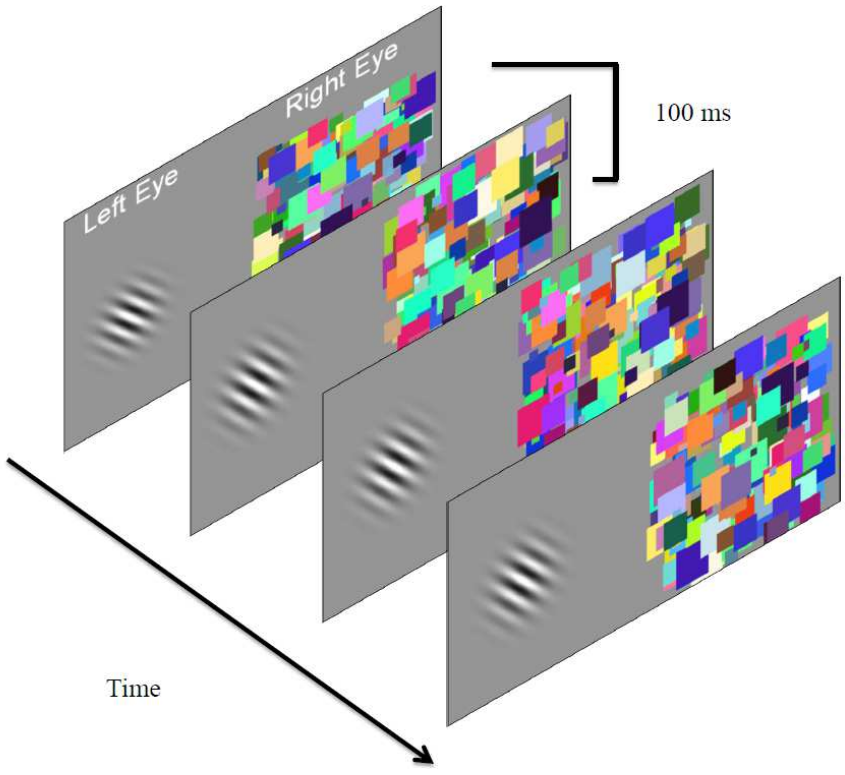

Figure 1.1. An example of continuous flash suppression (CFS). Here, an oriented Gabor patch is presented to the left eye, while the typical "Mondrian mask" consisting of squares of random size, position, and color is presented to the right eye. The CFS mask is typically refreshed every 100 ms (i.e., for all individual elements a new random position is generated).

Studying unconscious processing with CFS

To measure unconscious processing of a stimulus, CFS has been used in two different ways, referred to as direct and indirect measures of unconscious processing. In the latter case, a stimulus is presented in the absence of awareness and one measures subsequent adaptation aftereffects (Adams, Gray, Garner, & Graf, 2010; Bahrami, Carmel, Walsh, Rees, & Lavie, 2008a, 2008b; Kanai, Tsuchiya, & Verstraten, 2006; L. Kaunitz, Fracasso, & Melcher, 2011; Maruya, Watanabe, & Watanabe, 2008; Moradi, Koch, & Shimojo, 2005; Stein, Peelen, & Sterzer, 2012; Stein & Sterzer, 2011; Sweeny, Grabowecky, & Suzuki, 2011), attentional shifts elicited by the suppressed stimulus (Jiang, Costello, Fang, Huang, & He, 2006; Palmer & Ramsey, 2012), or priming effects (Almeida, Mahon, & Caramazza, 2010; Almeida, Mahon, Nakayama, & Caramazza, 2008; Bahrami et al., 2010; Barbot & Kouider, 2011; Sakuraba, Sakai, Yamanaka, Yokosawa, & Hirayama, 2012). An advantage of these measures is that they follow the classic dissociation logic in that a direct (subjective or objective) measure of stimulus awareness (of the suppressed stimulus) is contrasted with an indirect measure of stimulus processing (i.e., all the measures described above). A particularly tricky aspect of these experiments, however, is how to properly measure awareness of the suppressed stimulus. Traditional "objective" sensitivity measures such as d' have been criticized as a proper measure for assessing stimulus awareness (Amihai, 2012; Vermeiren & Cleeremans, 2012). Subjective measures such as the Perceptual Awareness Scale (Ramsøy & Overgaard, 2004), in which participants have to indicate their awareness of the suppressed stimulus on a 4-point scale on a trial-by-trial basis, may prove to be a potential solution, but these have their downsides as well. Depending on how effective suppression is, the consequence often is that up to half of the data needs to be removed due to some partial awareness of the stimulus on some trials.

Therefore, the direct measure to assess unconscious processing during CFS, the breaking CFS (b-CFS) paradigm, has gained more popularity (Gayet, Van Der Stigchel, & Paffen, 2014). In b-CFS, a trial typically starts with presenting a stimulus initially at a low contrast such that it will be perceptually suppressed. Subsequently, the contrast of the suppressed stimulus is gradually increased. The time it takes for the stimulus to break suppression (i.e., become detectable) is used as an index of unconscious processing. The task participants have to perform usually pertains to an aspect of the stimulus orthogonal to the

stimulus manipulation (e.g., location). The reasoning behind this paradigm is that differential suppression times for different stimuli must be due to some kind of unconscious representation of the stimulus that has been built up during suppression biasing the interocular competition process and yielding differential suppression times (Jiang, Costello, & He, 2007). Therefore, these suppression times are taken as an index of unconscious processing. b-CFS has been criticized, however, as a paradigm to study unconscious processing. That is, suppression times rely on detecting an aspect of the stimulus as soon as it reaches awareness. Therefore, differences between suppression times might be attributable to criterion differences related to the detection of the stimulus (i.e., different stimuli break suppression at the same time on average, yet have differential detection or decision criteria). To counter this, a binocular control condition in which the target and mask are presented to both eyes has been used. The absence of a difference between "suppression times" in this control condition is then argued to preclude the possibility that differential suppression times in the CFS condition are due to criterion differences in detection. However, because the target and mask are presented to both eyes in this condition, and no interocular suppression is elicited, it has been argued that this condition is too different from the CFS condition to unambiguously provide evidence for unconscious processing (Stein, Hebart, & Sterzer, 2011; Stein & Sterzer, 2014).

Perceptual organization and continuous flash suppression

The relationship between perceptual organization and visual awareness in general paints a complicated picture, with the results often being contingent on the exact paradigm used to render stimuli non-conscious and with many different perceptual organizational processes being involved. From these studies, it is probably fair to conclude, however, that there is evidence that some forms of perceptual organization still take place for stimuli rendered invisible through attentional manipulations (Gillebert & Humphreys, 2015; Kimchi, 2009; Moore & Egeth, 1997; Pitts, Martínez, & Hillyard, 2012; Vandenbroucke, Fahrenfort, Sligte, & Lamme, 2014). Likewise, there is also some evidence that certain forms of perceptual organization still manifest themselves in the absence of awareness (Mitroff & Scholl, 2005; Montoro, Luna, & Ortells, 2014; Norman, Heywood, & Kentridge, 2013; Poscoliero, Marzi, & Girelli, 2013).

The appealing properties of CFS as a blinding paradigm, the relative scarcity of its use to study invisible stimuli in the context of perceptual organization, and the fact that the suppression mechanisms of CFS remain poorly understood have shaped the focus of the studies presented in this dissertation. As will be apparent, only some chapters explicitly focus on classic perceptual organization phenomena (i.e., Chapter 4 on figure-ground organization, Chapter 6 on the perception of causality, Chapter 7 on biological motion). The motivation for the studies presented in the other chapters derived mainly from a range of interests in related topics. For example, in Chapter 5 we focus on whether perceptually suppressed stimuli are represented such that they could be integrated with auditory stimuli. In Chapter 8, we consider whether we could obtain evidence for a "natural input" hypothesis governing differences in suppression times (although this hypothesis is only spelled out in the General Discussion of this dissertation), Last, Chapters 9 and 10 consider whether some stunning findings on semantic processing of invisible stimuli could be replicated. As will be apparent from the results reported in these chapters, these studies also provide insight on the level up to which perceptually suppressed stimuli are processed. The last type of studies reported in this dissertation pertains to the study of CFS itself. That is, it is particularly important to understand the characteristics and mechanisms of a blinding paradigm. Upon its introduction, Tsuchiya and Koch (2005) interpreted CFS as a mix of binocular rivalry and flash suppression (hence, its name). Little work has been done to further validate this stance. Therefore, in Chapters 2 and 3, we explore the extent to which CFS is actually distinct from binocular rivalry by focusing on the importance of the spatial properties of the CFS mask in Chapter 2 and the structure of the sequence of suppression durations obtained in a typical b-CFS experiment in Chapter 3.

Due to the heterogeneous nature of the studies presented in all chapters, it might be useful to rephrase our original, general research question as a more specific one, in the light of which all studies - to some extent – provide an answer: *"What is the representation of a visual stimulus that is perceptually suppressed through continuous flash suppression, with a focus on perceptual organization?"*

The structure of this dissertation

There is no logical build-up from Chapters 2 to 10, neither with respect to complexity or content, nor with respect to chronological order. Rather, this dissertation can be considered to consist of two parts. Chapters 2 and 3 can be considered as Part One, in which we study the mechanisms of CFS itself. Part Two – Chapters 4 to 10 – consists of experimental studies that can be captured under the umbrella of "the fate of the suppressed stimulus". The ordering of these chapters could be considered as going from "mid-level" to "high-level" processing, yet imposing such a structure would do no justice to the continuous, parallel, and dynamical nature of the mechanisms involved in the phenomena being considered.

The last section of this Introduction summarizes some thoughts on contemporary statistical practice in psychology, and science more generally. It also includes a motivation and description of the statistical framework that is used in nearly every chapter of this dissertation, Bayesian statistics. The reader may freely skip this section if he/she is familiar with these methods, or has no interest in statistics whatsoever.

A note on statistical practice

Let us start with a bold statement: I think contemporary statistical practice in psychology has led us astray, has reduced theorizing and experimenting to the dull and banal question "but is it significant?" (causing a huge file drawer, Rosenthal, 1979), and is one of the primary causes of the so-called "crisis of confidence" (Button et al., 2013; Ioannidis, 2005; Pashler & Harris, 2012; Pashler & Wagenmakers, 2012).

In psychology and other scientific disciplines more generally, the predominant framework to perform statistical inference is a frequentist null hypothesis significance testing (NHST) framework. Here, one formulates a so-called "null" hypothesis on a certain parameter (often the population mean or difference between population means). The parameter value associated with the null hypothesis can in principle take on any value, but in practice it is nearly always zero ("the null ritual", Gigerenzer, 2004). Depending on the directionality of the predictions, an alternative hypothesis is then formulated in which the parameter is different from, smaller or larger than the value associated with the null (for a thought-provoking discussion of the implications of making directional predictions in the

context of this statistical framework, see Meehl, 1967). Next, one formulates a test statistic (e.g., z-statistic or t-statistic) of which the sampling distribution is derived based on the assumption of infinitely repeating the experiment, assuming that the null hypothesis is true, and formulating some distributional assumptions on the data (Fisher, 1925; Student, 1908). The observed statistic is then compared to the sampling distribution by calculating the probability to observe a more extreme value than the observed one, referred to as the p-value. If this p-value exceeds a threshold (often set at .05 – although it is difficult to pinpoint the actual origin of this consensus, see http://www.jerrydallal.com/LHSP/p05.htm), it is agreed upon to reject the null hypothesis of no difference and to infer that there is, for example, evidence for a difference between the means of the dependent variables in two experimental conditions. Unfortunately, the framework is intrinsically asymmetric with respect to hypothesis testing. That is, it is only possible to reject the null hypothesis, rather than also to accept it (Gallistel, 2009; Meehl, 1978; Rouder, Speckman, Sun, Morey, & Iverson, 2009). It is interesting to note that this statistical procedure or "null ritual" is actually a hybrid form of the procedures originally developed by Fisher on the one hand and Neyman and Pearson on the other hand (Gigerenzer, 2004). A second curiosity is that NHST has not only been the subject of criticism for the last decade or so (since the rise of the "Bayesian movement"), but for over 50 years already (Bakan, 1966; Cohen, 1994; Lykken, 1968; Meehl, 1967; Rozeboom, 1960; Wagenmakers, 2007). As a psychologist, it is a particular revealing example of how an academic community – smart and critical people – can remain deaf to these issues. I think some of these issues associated with p-value based inference are worth making explicit: (1) p-values do not teach us what we want to know, they quantify p(data | hypothesis), rather than p(hypothesis | data), (2) p-values depend on unobserved data, (3) p-values depend on potentially unknown subjective intentions (e.g., "optional stopping"), (4) p-values overstate the evidence against the null, (5) p-values do not allow the collect evidence in favor of the null. To paraphrase Jeffreys (1961): *"What the use of P implies, therefore, is that a hypothesis that may be true may be rejected because it has not predicted observable results that have not occurred."*

One might wonder why I have chosen to reflect on these issues in the context of this dissertation. Apart from my genuine interest in statistics, I think it is particularly important to understand that this statistical framework runs short of providing us the answers we are

genuinely interested in, not only in this dissertation, but in consciousness research more generally. Especially with respect to the study of unconscious visual processing, one needs to be equipped with the tools that allow us to quantify evidence in favor of or against unconscious visual processing (operationalized in whatever way). To do so, we need to have the tools at our disposal to explicitly compare two statistical models, allowing us to quantify the evidence in favor for either of them. Bayesian statistics provides a coherent framework to address this question, although we should stress from the start that any model comparison approach suffices in this context – be it frequentist, Bayesian, or resampling methods (e.g., an interesting example of model comparison in a frequentist context is provided in Rouder et al., 2009). A primary advantage of Bayesian statistics is that any inference is conditional on the observed data rather than hypothetical replications of the data, as in frequentist inference. Because it provides such an attractive alternative to classical frequentist statistics, we have made the principled decision to use this statistical framework throughout the dissertation (except for Chapter 3, where we used nonparametric measures). Given that not everyone might be familiar with these methods, we provide a short introduction here and refer to more specialized books and papers for a more in-depth introduction to and discussion of these methods (Kruschke, 2010, 2011, 2013; Rouder, Morey, Speckman, & Province, 2012; Rouder et al., 2009; Wagenmakers, 2007).

As in every statistical framework, Bayesian statistics provides the opportunity to do parameter estimation or hypothesis testing. Parameter estimation relies on the posterior distribution over the parameters of interest, whereas hypothesis testing relies on Bayes Factors. For both purposes, Bayes' rule is crucial:

$$p(\theta|D) = \frac{p(D|\theta)p(\theta)}{p(D)}$$

where θ refers to a vector of parameters (e.g., the effect parameters of an ANOVA model) and D to the observed data. In Bayes' rule, the prior probability distribution, $p(\theta)$, is then updated by the likelihood $p(D|\theta)$ to yield the posterior probability distribution, $p(\theta|D)$. Because the normalizing constant or marginal likelihood, $p(D)$, often involves computing an intractable integral in the case of complex models, the use of Bayesian methods was limited to simple statistical models that were analytically tractable. In this respect, the advent of

software that allows to sample from the posterior distribution by relying on Markov Chain Monte Carlo (MCMC) sampling has been a major contribution to the field (Lunn, Thomas, Best, & Spiegelhalter, 2000; Plummer, 2003; Stan Development Team, 2016). With fairly limited background knowledge, it is now possible to fit complex models in a Bayesian framework (for a great hands-on introduction, see Lee & Wagenmakers, 2014).

In this dissertation, we mostly rely on model comparison rather than parameter estimation, by using Bayes Factors (Jeffreys, 1961; Kass & Raftery, 1995). In the Bayes Factor approach, the focus is on the marginal likelihood of Bayes' rule, $p(D)$:

$$p(D) = \int p(D|\theta)p(\theta)d\theta$$

The Bayes factor then refers to the ratio of marginal likelihoods of different statistical models under consideration (e.g., a model with main effects of congruency and inversion versus a model with only a main effect of congruency), quantifying the change from prior to posterior model odds:

$$\frac{p(M_1|D)}{p(M_2|D)} = \frac{p(M_1)}{p(M_2)} \frac{p(D|M_1)}{p(D|M_2)}$$

where

$$BF_{12} = \frac{p(D|M_1)}{p(D|M_2)} = \frac{\int_{\Theta_1} p(D|\theta)p(\theta)d\theta}{\int_{\Theta_2} p(D|\theta)p(\theta)d\theta}$$

In itself, the Bayes Factor can only be interpreted as a *relative* measure of evidence for one statistical model compared to another (e.g., a model with two main effects versus a model with two main effects and their interaction). That is, the value of the Bayes Factor should always be interpreted relative to the statistical models under consideration. Because the prior distribution is integrated out in the marginal likelihood, the Bayes Factor automatically penalizes for model complexity (i.e., in more complex models the probability

density will be more "spread out"). Therefore, it is sometimes referred to as the automatic Occam's razor.

The Bayes Factor is an inherently continuous measure (bounded between zero and infinity), yet some cut-off values are sometimes used as a guideline for interpretation. A Bayes Factor of 3 (or 1/3) is frequently used as positive or substantial evidence in favor of or against the alternative hypothesis. Bayes Factors > 10 (or < 0.1) are considered to indicate strong evidence for either model. In this dissertation, we use these cut-off values as guidelines to interpret our results, yet it should be stressed from the outset that lumping together Bayes Factors into categorical interpretations does no justice to the fact that it is a continuous measure of evidence.

A plea for open software

The last couple of years, a lot of voices have been raised to increase the reproducibility of our research. There is an important distinction between reproducibility and replicability. That is, reproducible research refers to the fact that someone can reproduce the results you reported by redoing the analyses on your data. Replicable research refers to studies of which the findings can be replicated independently, but for which the numbers can vary due to a host of reasons (Leek, Patil, & Peng, 2015). Because reproducible research can hardly be achieved when research colleagues have to use commercial software which might not always be at their disposal, we have always relied as much as possible on open source software throughout this dissertation. For programming experiments, we have always used Python 2.7 and the PsychoPy software package (Peirce, 2007, 2009). For data analysis and visualization, we have always relied on R (R Core Team, 2014) and RStudio (RStudio Team, 2015). In particular, the following packages have been of great value: plyr (Wickham, 2011), dplyr (Wickham & Francois, 2015), lme4 (Bates, Maechler, Bolker, & Walker, 2015), BayesFactor (Morey & Rouder, 2015), and many more. Because the creators of these software packages all too often are not sufficiently acknowledged, I would like to personally thank them for making my life easier, *a lot* easier.

Chapter 2.
Moving stimuli are less effectively masked using traditional Continuous Flash Suppression (CFS) compared to a Moving Mondrian Mask (MMM): a test case for feature-selective suppression and retinotopic adaptation.

Continuous flash suppression (CFS) is a powerful interocular suppression technique, which is often described as an effective means to reliably suppress stimuli from visual awareness. Suppression through CFS has been assumed to depend upon a reduction in (retinotopically specific) neural adaptation caused by the continual updating of the contents of the visual input to one eye. In this study, we started from the observation that suppressing a moving stimulus through CFS appeared to be more effective when using a mask that was actually more prone to retinotopically specific neural adaptation, but in which the properties of the mask were more similar to those of the to-be-suppressed stimulus. In two experiments, we find that using a moving Mondrian mask (i.e., one that includes motion) is more effective in suppressing a moving stimulus than a regular CFS mask. The observed pattern of results cannot be explained by a simple simulation that computes the degree of retinotopically specific neural adaptation over time, suggesting that this kind of neural adaptation does not play a large role in predicting the differences between conditions in this context. We also find some evidence consistent with the idea that the most effective CFS mask is the one that matches the properties (speed) of the suppressed stimulus. These results question the general importance of retinotopically specific neural adaptation in CFS, and potentially help to explain an implicit trend in the literature to adapt one's CFS mask to match one's to-be-suppressed stimuli. Finally, the results should help to guide the methodological development of future research where continuous suppression of moving stimuli is desired.

Moors, P., Wagemans, J., & de-Wit, L. (2014). Moving stimuli are less effectively masked using traditional Continuous Flash Suppression (CFS) compared to a Moving Mondrian Mask (MMM): A test case for feature-selective suppression and retinotopic adaptation. PLoS One, 9 (5), art.nr. e98298.

INTRODUCTION

Since Crick and Koch (1998) set out a framework for studying the neural correlates of consciousness, a number of paradigms have been developed to study the neural activity associated with purely perceptual changes that allow one to study the correlates of consciousness without changing the visual input. Continuous flash suppression (CFS) is a psychophysical technique that enables this by suppressing stimuli much more reliably than in standard binocular rivalry paradigms, and with much longer presentation times possible compared to visual masking paradigms (Tsuchiya & Koch, 2005). In essence, CFS is highly similar to binocular rivalry: Two different images are presented to the same regions of both eyes, but in one eye, a rapidly changing stimulus is presented, which effectively suppresses the stimulus in the other eye for relatively long periods of time (i.e., units of seconds rather than seconds, Tsuchiya & Koch, 2005). Traditionally, this changing stimulus is a Mondrian fashioned pattern of rectangles and squares of random size and color that changes every 100ms (10 Hz).

Since CFS was introduced as a technique to reliably suppress stimuli from visual awareness, it has been used in more than 100 studies. In these experiments, CFS has been applied in two different ways. First, it has been used to present stimuli in the absence of awareness and to study the influence of the presentation of these subliminal stimuli on a subsequent task with visible stimuli. For example, Jiang, Costello, Fang, Huang, and He (2006) report an attentional effect of unconsciously presenting erotic pictures. CFS has been used in this way to study the orientation aftereffect (Bahrami et al., 2008a, 2008b), motion aftereffect (Kaunitz et al., 2011; Maruya et al., 2008), simultaneous motion contrast (Kawabe & Yamada, 2009), face adaptation (Adams et al., 2010; Moradi et al., 2005; Yang, Hong, & Blake, 2010), as well as a preconscious attentional bias in cigarette smokers (Yan et al., 2009).

Secondly, CFS has been most often put into practice in the so-called "breaking CFS paradigm" (a term coined by Stein, Hebart, et al., 2011, based on the paradigm introduced by Jiang, Costello, & He, 2007). In this paradigm, participants have to detect when a stimulus suppressed through CFS breaks through the mask. Differential breakthrough times for different conditions are then taken as evidence that some kind of unconscious representation of the different stimuli must have been generated in order for them to break through at differential rates. Using this technique (abbreviated as "b-CFS"), Jiang et al. (2007) showed

that faces break through suppression faster than inverted faces. Since b-CFS was introduced, it has been used widely. Costello, Jiang, Baartman, McGlennen, and He (2009) provided evidence for unconscious semantic word priming, Bahrami et al. (2010) for unconscious numerical processing, Xu, Zhang, and Geng (2011) for gaze cuing in the absence for awareness, Wang, Weng, and He (2012) for perceptual grouping of a Kanizsa triangle under CFS and Mudrik, Breska, Lamy, and Deouell (2011) documented that incongruent scenes break through faster than congruent scenes.

Despite the broad and increasing employment of this method, it is still is not clear which factors contribute to the effectiveness of CFS in suppressing stimuli from awareness. Some authors imply that the effectiveness of CFS derives from its saliency. For example, Bahrami, Lavie, and Rees (2007) describe their CFS mask as "highly salient, high-contrast, and rapidly changing blue masks" (p. 510); Raio, Carmel, Carrasco, and Phelps (2012) refer to a "salient dynamic stimulation" (p. R477). Other authors (Faivre, Berthet, & Kouider, 2012; Hesselmann, Hebart, & Malach, 2011; Hesselmann & Malach, 2011; Seitz, Kim, & Watanabe, 2009; Shimaoka & Kaneko, 2011; Yamada & Kawabe, 2011, 2012) describe their CFS masks similarly. The most widespread explanation for the effectiveness of CFS, however, has been a general reduction in neural adaptation due to the fast transients associated with the mask (as in Tsuchiya et al., 2006; Yang & Blake, 2012). That is, the input at a (retinotopic) location is updated every ~100ms, causing neurons with a receptive field at that location to show less neural adaptation compared to static input. Indeed Tsuchiya et al. (2006) say: "We imagine that the enduring effectivess of CFS arises from its relative immunity to adaptation owing to the repeated presentation of a new stimulus" (p. 1075). Yang and Blake (2012) are more explicit and articulate: "Perhaps, then, the rapid, repetitive changes in the successively presented, random configurations of a CFS display minimize its tendency to undergo neural adaptation (…)" (p. 11). Thus, because at every retinotopic location features such as orientation and contrast change, neurons responsive for these features tend to adapt less compared to static input. In this sense CFS can be understood as a form of binocular rivalry, in which percept switches have been explained (in part) as the result of neural adaptation to the dominant percept and competition between monocular neurons in low-level visual areas (Alais, 2012; Blake, 1989). Since the interocular competition process in binocular rivalry has

mostly been characterized as happening in low-level visual areas (although recent models acknowledge the possibility for competition between different levels in the hierarchy of the visual system, Blake & Logothetis, 2002; Tong et al., 2006), we focus on the extent to which retinotopically specific neural adaptation can help to predict the effectiveness of CFS (see the General Discussion for further discussion of the role of higher-order adaptation processes in CFS). Framing the effectiveness of CFS as preventing retinotopically specific neural adaptation due to these fast changes in the mask would imply that the more changes over time the mask contains, the more effective CFS should be. Indeed, this assumption also appears to be implicit in the literature when the refresh rate of the CFS mask is changed. Although most authors continue to use the traditional 10Hz refresh rate as suggested in Tsuchiya and Koch (2005), when they do not, the refresh rate is mostly increased. Of the 81 studies we considered, 72% used the canonical 10Hz refresh rate and 20% employed a refresh rate of more than 10Hz. Indeed, Xu et al. (2011) increased the refresh rate with the explicit assumption that this would lead to more robust interocular suppression than the traditional 10Hz frequency.

This explanation in terms of a reduction to retinotopically specific neural adaptation provides no immediate explanation for the way in which the traditional Mondrian mask is often adapted in the literature when masking specific stimuli in different studies. Different authors seem to adapt the characteristics of the CFS mask to match the characteristics of the to-be-suppressed stimulus. We provide three illustrative examples. First, Stein, Hebart, et al. (2011) note that, although regular CFS allows for masking faces, it is much more effective to mask faces with a mask consisting of ellipses instead of squares. Second, the study of Bahrami et al. (2007) used random geometrical shapes, contours and moving dots to suppress line drawings. Again, this adapted mask appears more similar to the to-be-suppressed line drawings than the standard Mondrian mask. Third, Sweeny et al. (2011) used a mask of randomly generated non-filled ellipses to mask an open or closed curve.

Only recently the importance of the characteristics present in the CFS mask has been highlighted as an important factor with respect to the effectiveness of CFS (Hong & Blake, 2009; Maehara, Huang, & Hess, 2009; Yang & Blake, 2012). Indeed, Tsuchiya and Koch (2005) never explicitly motivated their choice for the rapidly changing and flickering Mondrian-style rectangles as an effective CFS mask. Yang and Blake (2012) proposed to address this

issue by studying the effectiveness of CFS in relation to the properties of the mask and the suppressed stimulus. With respect to the spatial properties of the mask and suppressed stimulus, their results show that (1) it is harder to mask stimuli with high spatial frequency properties and (2) that stimuli with low spatial frequency properties are most effectively masked with CFS masks containing mostly low spatial frequencies.

Along the same line, an earlier study by Maehara et al. (2009) reported almost no difference between suppression strength of a static and a flickering mask in suppressing a target stimulus when the spatial frequencies of mask and target were at least 1.6 octaves away from each other. Based on this result, Maehara et al. (2009) proposed that the effectiveness of CFS presumably stems from within-channel masking.

Thus, it seems to be the case that the depth of suppression during CFS is not fixed, but rather depends on the interaction between the characteristics of the mask with the suppressed stimulus. This perhaps reintegrates our understanding of CFS with existing studies of binocular rivalry in general concerning feature-selectivity and the effect of shared stimulus complexity on suppression strength (e.g., Alais & Melcher, 2007; Alais & Parker, 2006; Stuit, Cass, Paffen, & Alais, 2009).

The potential importance of feature-selective competition in CFS does not rule out a role for adaptation-based explanation of its effectiveness. Indeed, for all these examples it is hard to disentangle the contribution of retinotopic neural adaptation and feature selectivity. In the present study we explore the relative contributions of retinotopic adaptation and feature competition by manipulating the properties of the CFS mask in such a way that feature overlap with the suppressed stimulus and retinotopic adaptation can be disentangled.

The Present Study

As already highlighted, CFS is commonly described as a highly effective technique to suppress stimuli from visual awareness reliably and for longer time periods (Tsuchiya & Koch, 2005; Tsuchiya et al., 2006). Since CFS potentially offers long suppression times, it provides an excellent opportunity to be used in the context of suppressing dynamic stimuli that change over time (e.g., random-dot motion, biological motion, etc.). During pilot testing, however, we observed that regular CFS did not provide an effective means of suppressing

moving stimuli. Rather, it appeared that introducing spatiotemporal continuity (e.g., motion) into our CFS-style mask seemed to be required to provide useful suppression times. The need to introduce continuous spatiotemporal signals into the mask does not seem to logically follow from what would be predicted from an account based on a reduction to retinotopically based neural adaptation in driving the effectiveness of CFS. Indeed, the spatiotemporal continuity we used to develop effective suppression should, if anything, be more prone to retinotopic neural adaptation than regular CFS. Given this observation, we set out to test whether a moving Mondrian mask (MMM) indeed provides a better means of suppressing a simple moving stimulus compared to regular CFS. In order to formally test the potential importance of retinotopic neural adaptation we explicitly operationalized the term and implemented a simple computation to quantify the degree of retinotopically specific neural adaptation (see Methods & Results of Experiment 1). Thus, the goal of this study was to show that, for moving stimuli, a release from retinotopically specific neural adaptation due to the changes in the mask over time is not the only mechanism that drives the effectiveness of CFS nor is it potentially the most dominant or important in a given context (Maehara et al., 2009; Yang & Blake, 2012).

To preview our results, our formalization of retinotopic neural adaptation proved to provide no basis for predicting the suppression strength of different MMMs containing different motion speeds.

EXPERIMENT 1

In the first experiment, we used a MMM, manipulated the speed of the individual mask elements, and compared its effectiveness to a regular CFS mask in suppressing a moving target. Subsequently, we compared the observed effectiveness with what would be predicted to be the most effective mask based on a measure of retinotopically specific neural adaptation.

Materials and Methods

Ethics Statement. The study was conducted in line with the ethical principles regarding research with human participants as specified in The Code of Ethics of the World Medical Association (Declaration of Helsinki). The study was approved by the Ethical

Committee of the Faculty of Psychology and Educational Sciences (EC FPPW) of the University of Leuven, and the participants gave written informed consent before starting the experiment.

Participants. Five students (1 male) of the undergraduate psychology program of the University of Leuven participated in the experiment for course credit. All had normal or corrected-to-normal vision. Every participant was unaware of the goal of the study.

Apparatus. Stimuli were shown on two 19.8-in. Sony Trinitron GDM F500-R (2048 x 1536 pixels at 60 Hz, for each) monitors driven by a DELL Precision T3400 computer with an Intel Core Quad CPU Q9300 2.5 GHz processor running on Windows XP. Binocular presentation was achieved by a custom made stereo set-up. Two CRT monitors, which stood opposite to each other (distance of 220 cm), projected to the left and right eye respectively via two mirrors placed at a distance of 110 cm from the screen. A head- and chin rest (15 cm from the mirrors) was used to stabilize fixation. The effective viewing distance was 125 cm. Stimulus presentation, timing and keyboard responses were controlled with custom software programmed in C# using Microsoft Visual Studio 2010.

Stimuli. A checkerboard pattern consisting of randomly positioned black and white squares of 0.37° by 0.37° was used to aid binocular fusion. The CFS masks consisted of 150 squares of equal size (0.46° by 0.46°). The color of the squares was either red, green, blue or yellow. The target stimulus was a red circle (diameter 0.46°). The target moved horizontally across a virtual square (5.5° by 5.5°) at a speed of 3°/s embedded in a larger square (7.32° by 7.32°). The mask was presented in the other eye within a bounding square of the same size as the larger square in the other eye (7.32° by 7.32°, see Figure 2.1).

The MMM differed from the traditional CFS mask. The main difference was that the individual elements of the mask were moving from frame to frame rather than flashing at randomly generated positions. Motion in the mask varied in six different directions (horizontal left/right and right/left, vertical up/down and down/up, diagonal bottom-left/top-right and top-left/bottom-right). Every mask element had one of these motion directions during the trial and the different motion directions were equally divided amongst

the mask elements such that every motion direction was equally present in the display. For every motion direction, the colors of the individual elements were also evenly distributed. The initial position of every mask element was determined randomly with one constraint. To avoid that some parts of the display did not contain enough mask elements during a trial (creating blank spots), we divided the display into four quadrants and the positions, speeds and colors for each fourth of the mask elements were randomized within this quadrant. The size of the individual mask elements was the same as the size of the suppressed stimulus. When the positions of the mask elements overlapped, they were drawn on top of one another. Furthermore, when a mask element reached the edge of the display, it would reappear on the other side according to its motion trajectory.

Procedure. On each trial, the CFS mask was shown in the participant's dominant eye. Eye dominance was determined prior to the start of the experiment with Porta's test (Porta, 1593). Consequently, the target stimulus was presented in the non-dominant eye. The target stimulus and the MMM/CFS mask would onset simultaneously, but the target began at a low contrast level, and faded in during the first 20 frames of the event. The target stimulus moved on a horizontal plane from the right side to the left and disappeared from the screen after 3.6 seconds. The target moved either above or below fixation at one of six motion paths (three above and three below fixation) randomly selected on every trial (but balanced across the motion conditions). These motion paths were equally spaced from each other. The distance between every of the three different motion paths was twice the target size. After the target disappeared from the screen, participants had to indicate if the target moved above or below the fixation cross. Contrast thresholds for the different mask speeds were determined by a one-up, two-down staircase procedure converging at 70.71% correct (Levitt, 1971). Two staircases were used for every mask speed. This resulted in twelve randomly interleaved staircases. The targets for the two staircases where given different starting values, in order to ensure the convergent consistency of the resulting thresholds. Because the task often was too easy for the high starting values, these staircases were accelerated by using a one-up, one-down procedure until the first incorrect response was recorded (for each of these staircases).

Design. Mask speed consisted of six different levels (1°/s, 2°/s, 3°/s, 5°/s, 8°/s and regular CFS) and two staircases were used for each mask speed. Participants performed 65 trials for each staircase, resulting in 780 (65 trials x 6 speeds x 2 staircases) trials in total. The number of trials per staircase was selected based on pilot testing. Staircases were randomly interleaved and participants had the opportunity to take small breaks in-between.

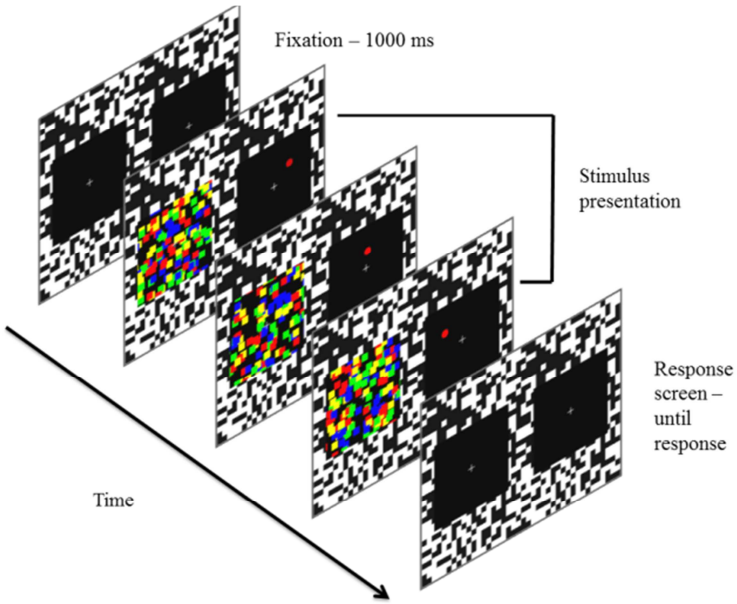

Figure 2.1. An example of the trial sequence.

Simulations. Since we were interested in quantifying the degree of retinotopic neural adaptation for the different masks in our experiment, we conducted a simulation which implemented an approximation of the retinotopic representation of the input using Gabor filter banks often used in models of the early visual system (e.g., Riesenhuber & Poggio, 1999). As highlighted above, the effectiveness of CFS has most often been explained as a reduction in neural adaptation due to the successive presentations of new random configurations. In order to provide a more explicit model of this how this adaptation process might work in early retinotopic areas, we convolved the stimuli in our experiment with a Gabor filter bank to extract orientation- and contrast-selective responses at each location of the image (akin to responses of neurons in primary visual cortex) and then used these responses as input to an adaptation simulation. An exponential decay function was used to represent adaptation to the input, and an exponential recovery function to represent the

recovery of that retinotopic location when no input was present. These functions had the following form for decay and recovery, respectively:

$$y = k_1 + k_0 e^{-t/\tau}$$

$$y = k_1 (1 - k_1) e^{-t/\tau} + k_0$$

where k_0 is the initial response level set at 1, k_1 the asymptotic response level set at 0 for decay and 1 for recovery, and τ the time constant for the exponential. Note that this is not necessarily intended to provide a full or complete 'model' of retinotopic adaptation. Rather this simulation intends to make explicit what a simple approximation of retinotopic adaptation could look like. It is certainly possible that the adaptation dynamics in early retinotopic areas are much more complex, but this simulation enables us to quantify whether this very basic approximation of retinotopic adaptation can already predict our current results.

We simulated 999 trials of each condition and transformed each frame of the trial to a grayscale image. Next, we filtered each frame with two oriented (at 0 and 90 degrees) odd-symmetric Gabor filters with a spatial frequency set at the Nyquist frequency (412 c/image) and the standard deviation of the Gaussian set to:

$$\sigma = \lambda \frac{1}{\pi} \sqrt{\frac{\ln(2)}{2}} \frac{2^b + 1}{2^b - 1}$$

where λ equals the wavelength in pixels and b the bandwith in octaves. These settings were chosen to efficiently extract the responses to the edges of the different configurations in the CFS mask. The size of the filters was set to be four times the standard deviation of the Gaussian (varying filter size had no effect on the filter responses). This filtering procedure was implemented in the Python MDP package (Zito, Wilbert, Wiskott, & Berkes, 2009). For each frame, this filtering step yields orientation- and contrast-specific responses for each pixel in the image, thresholded to be one out of five responses (2 orientations times 2 polarities and no response). Next, we calculated the degree of adaptation for each pixel by letting an "activation value" (starting at 1) decay with a time constant of four seconds as long as the input was present. When the input was no longer present, this "activation value" would recover again with a time constant of six seconds and this process would go on until the end of the trial. Note that this adaptation process was specific to one of the four possible filter responses. Both orientation and contrast polarity had to be the same

across frames to yield adaptation. The time constants for adaptation and recovery were based on Giaschi, Douglas, Marlin, and Cynader (1993). This implementation yields an activity map for each location in the image for each of the four possible responses and we summed the values across all locations in an image to arrive at one summary statistic for the activation level associated with each mask condition. In other models of binocular rivalry, the output of the filtering step implemented here can be thought of as a representation of the 'strength' of the stimulus, which in these models is usually expressed as a single value. In most models of binocular rivalry the adaptation process is simulated on this one summary value of stimulus strength. In order to approximate the nature of adaptation on early retinotopic maps however we calculate a 'stimulus strength' value at every location (based on a Gabor filter), and apply adaptation at every location. In this way we try to isolate the contribution of 'retinotopic' adaptation to the effectiveness of CFS. The longer the extracted stimulus features remain the same at every retinotopic location, the more adaptation it will experience, thus the amount of adaptation at each location will be greater if the stimulus features remain the same over time. This should occur more in the slower motion conditions. The greater influence of adaptation in the slower motion conditions will result in a faster decrease in the 'stimulus strength' represented at every location, and this will be combined to influence the summary score. Thus, this summary score can be thought of as an (inverse) index of the degree of retinotopic adaptation, with more adaptation (associated with the slower motions) leading to a lower summary score.

Results

Each threshold was determined by taking the last 20 trials of the staircases. These trials were then averaged within every staircase and subsequently averaged over staircases within each mask speed. Because of large inter-individual differences between thresholds, we normalized the thresholds by dividing them with the mean of the thresholds per participant. These normalized thresholds were then subjected to a Bayesian version of a one-way within-subjects ANOVA. Statistical inference throughout this paper did not use the classical frequentist framework but rather a Bayesian framework (see Kruschke, 2010a for an introduction). Bayesian statistics offer a lot of advantages over the classical frequentist framework (Kruschke, 2010a, 2010b; Rouder et al., 2009; Wagenmakers, 2007), which has

been disputed ever since it was introduced in psychology (e.g. from Rozeboom, 1960 to Wagenmakers, 2007). Moreover, using Bayesian inference as the principal way to do statistical inference is taken up more and more by researchers in vision science (e.g., see Rolfs, Dambacher, & Cavanagh, 2013).

In our analyses, we first do model selection using Bayes Factors. Subsequently, we use Bayesian parameter estimation to further zoom in on the posterior distributions. In both cases (model selection and parameter estimation) we have used the tools that are currently available. Note that these tools rely on different models with different, but in both cases uninformative, priors. Techniques for an integrated Bayesian approach to both model selection and parameter estimation are currently quite complicated to implement.

Bayesian model selection. Rouder, Morey, Speckman, and Province (2012) developed an approach in which a default class of priors is used to compute Bayes Factors in ANOVA designs. For an introduction, we refer to their paper. The Bayes Factor comparing a model with no effect and one with an effect of mask speed was equal to 13,992 indicating convincing evidence for a main effect of mask speed. Note that a classical repeated measures ANOVA yielded the same conclusion ($F(5,20) = 6.205$, $p = .001$). Figure 2.2 depicts the mean normalized threshold per condition and shows that the condition in which the speed of the CFS mask matched the speed of suppressed stimulus is the one with the highest threshold. In the next section, we will further zoom in on the results using Bayesian parameter estimation.

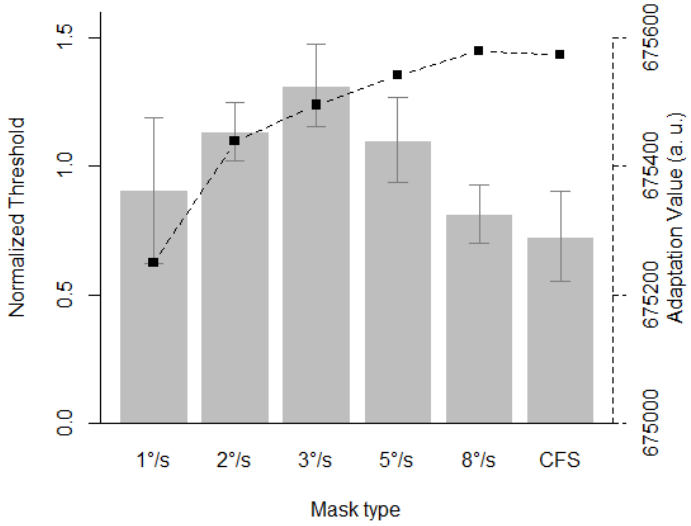

Figure 2.2. Mean normalized threshold in function of mask speed. The error bars denote 95% within-subject confidence intervals. The squares indicate simulated adaptation values (in arbitrary units) for each mask speed and regular CFS. Note that high values indicate *less* adaptation.

Bayesian parameter estimation. Figure S2.1 depicts the hierarchical model that was used to do Bayesian parameter estimation. This model was adapted from Gelman (2005) and Kruschke (2010a). The model is called hierarchical because it includes uncertainty at multiple levels. In contrast with classical repeated-measures ANOVA, the data were modeled as coming from a t-distribution instead of a normal to accommodate the possible influence of outliers. This method has also been called robust inference. Since the degrees of freedom of this t-distribution are unknown, it was treated as an unknown parameter and an uninformative uniform distribution was put on this parameter to let the data inform us about which degrees of freedom are in a credible range. The mean of the t-distribution is the result of a linear model (as in the classical repeated-measures ANOVA), comprising the general mean (β_0), the effect of mask speed (β_{1i}) and the subjects factor (β_{2j}). Furthermore, prior distributions are put on the parameters of the linear model. Note that these priors are not separate for each condition or subject, allowing that estimates for one condition inform estimates for the other or estimates for one subject are informed by estimates from other subjects. This is only one example of the flexibility of the Bayesian data-analytic approach

and the advantage is that one has to be explicit about the assumptions included in the model that is used to analyze the data. Markov Chain Monte Carlo (MCMC) sampling was used to generate samples from the posterior distribution using the JAGS software.

Since the posterior distributions for β_{1i} are deflections away from the baseline, contrasts can be computed to examine differences between two or more conditions – note that this is similar to performing a t-test. Here, we computed the difference between 3°/s and the average of all other conditions as well as pairwise comparisons between 3°/s and the other conditions. Figure S2.2 shows the posterior distributions associated with these contrasts. The black lines indicate the 95% highest density interval (HDI). This 95% HDI can be interpreted as an interval of credible parameter values. If this interval includes zero, we conclude that the compared conditions are not different and vice versa when zero falls out of the 95% HDI. Note that we can compute all these contrasts and do not have to use a correction for multiple comparisons. Indeed, there is just one (high-dimensional) posterior distribution and it does not change when you examine it in different ways (Kruschke, 2010a, pp. 284-285).

In summary, the data suggest that the normalized threshold for a mask speed of 3°/s is credibly different from the average threshold of all other mask speeds. Furthermore, pair-wise comparisons suggest that this difference holds for a mask speed of 1°/s and 8°/s. As a sanity check, Figure S2.3 depicts a posterior predictive check. In a posterior predictive check, every sample from the MCMC chain is used to predict a new data point by generating a random sample from the distribution you assume the data are generated from. If the model used for analyzing the data is not a good model, this would become clear from the predictions based on the believable parameter values. That is, these would deviate from the data or show a trend that is not present in the data. From Figure S2.3 it is apparent that the model used for this data set is a good model in the sense that it generates data that are in the range of the observed data.

Comparison with the simulations. In the Introduction, we suggested that the effectiveness of CFS does not entirely depend on retinotopically specific neural adaptation

due to the continuous updates to the CFS mask. As the results of Experiment 1 indicate, the MMM that matched the motion properties of the suppressed target provided the most effective suppression. However, whilst it is logical to assume that the mask of 3°/s would show more retinotopically specific neural adaptation, it was important to quantify this explicitly, especially in relation to the traditional CFS mask. To address this, we computed a measure of the degree of retinotopic neural adaptation as described in the Methods section.

The squares in Figure 2.2 depict the results of the simulations. As is apparent from this figure, our implementation of retinotopically specific neural adaptation showed a continuous increase from the slowest to the fastest mask speed and regular CFS, where an increase indicates *less* adaptation (as explained in the Methods section). However, our results deviate from these simulations as an increase in thresholds up to 3°/s and a decrease in thresholds for masks with faster speeds was observed.

Discussion

In Experiment 1, we manipulated the properties of MMMs and compared their effectiveness in suppressing a moving stimulus with regular CFS. We compared our pattern of results with that expected based on computations of the degree of retinotopic neural adaptation. If avoidance of retinotopically specific neural adaptation underlies the effectiveness of CFS, the mask with the most changes would prove to be the most effective. According to our measure of degree of retinotopic neural adaptation, the MMM and the regular CFS mask would show the least adaptation. However, the fastest mask speeds did not prove to be the most effective. It was apparent that the contrast threshold was highest for a MMM that matched the motion properties of the suppressed stimulus providing evidence for feature-selective depth of suppression during CFS (i.e., in line with Maehara et al., 2009; Yang & Blake, 2012).

Given the seemingly widespread assumption that effective CFS masking is driven by robustness to (retinotopic) neural adaptation, we tried to replicate our finding from Experiment 1 using two different to-be-suppressed target speeds. Thus, in Experiment 2 we manipulated the speed of the suppressed stimulus to move at either 2°/s or 5°/s whilst keeping the same range of mask speeds used as in Experiment 1. This also enables us to test the role of feature-selective depth of suppression during CFS. Indeed, analogous to the

results of Yang & Blake (2012) and compared to the results of Experiment 1, one would predict that the peak in the contrast threshold would shift toward a CFS mask where the speed is matched at 2°/s or 5°/s respectively for targets moving at 2°/s and 5°/s.

EXPERIMENT 2

Materials and Methods

Participants. Six new participants (1 male), all students of the undergraduate psychology program of the University of Leuven participated in the experiment for course credit. All had normal or corrected-to-normal vision. Every participant signed an informed consent prior to the start of the experiment and was naive to the goal of the study.

Apparatus and Stimuli. The apparatus and stimuli were the same as in Experiment 1.

Design. Mask speed again consisted of six different levels (1°/s, 2°/s, 3°/s, 5°/s, 8°/s and regular CFS). Target speed was also manipulated and consisted of two levels (2°/s and 5°/s), yielding a 2 x 6 within-subjects design. Participants performed 65 trials for each staircase, resulting in 1,560 (65 trials x 6 speeds x 2 target speeds x 2 staircases) trials in total. Target speed was blocked and counterbalanced across participants. In every block, staircases were randomly interleaved and participants had the opportunity to take small breaks in-between.

Procedure. The procedure of Experiment 2 was the same as in Experiment 1. The targets moved on a horizontal plane from the right side to the left and disappeared from the screen after 5.5 and 2.2 seconds, respectively for the 2°/s and 5°/s target speed conditions.

Results

The data were analyzed in the same way as in Experiment 1. First, we report the results from Bayesian model selection and subsequently we elaborate on them using Bayesian parameter estimation.

Bayesian model selection. Bayes Factors were again computed based on Rouder et al. (Rouder et al., 2012). Different Bayes Factors are reported in Table 2.1, all of which can be interpreted as a comparison with a full model including the main effect of mask speed, the main effect of target speed and the interaction. Bayes Factors smaller than one indicate evidence for the full model.

Table 2.1. Bayes Factors associated with a comparison with the full model.

Model	**Bayes Factor**
Null	0.0003
Mask Speed + Target Speed	0.178
Target Speed + Mask Speed * Target Speed	0.0001
Mask Speed + Mask Speed * Target Speed	5.1146

As the table shows, the model with a main effect of mask speed and an interaction between mask speed and target speed is strongly preferred. Note that a classical repeated measures ANOVA yields a similar conclusion (main effect of mask speed: $F(5,25) = 4.066$, $p = .008$; no main effect of target speed: $F(1,5) = 1.346$, $p = .298$; interaction between mask and target speed: $F(5,25) = 2.156$, $p = .09$). As is apparent from Figure 2.3 and in line with our predictions, the data indeed shift for the condition in which the target moved at 2°/s. The pattern of results is more complicated for the condition of 5°/s, however. Here, the thresholds seem to "flatten out" when the target moves at this speed.

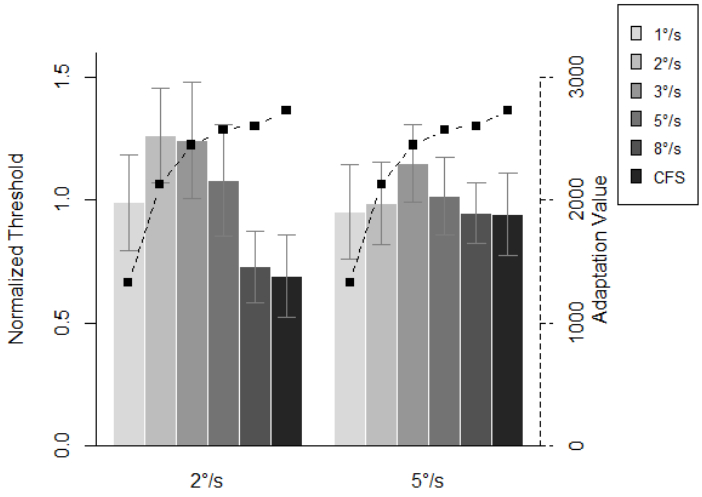

Figure 2.3. Mean normalized thresholds in function of target speed and mask speed. The error bars denote 95% within-subject confidence intervals. The squares indicate the simulated adaptation values (in arbitrary units) for each mask speed and regular CFS. Note that high values indicate *less* adaptation.

Bayesian parameter estimation. Parameter estimation was done with a similar hierarchical model as in the analysis of Experiment 1, but with an extra main effect – target speed – added to the model. Figure S2.4 shows the associated graphical model. Because of the interaction, pair-wise comparisons were computed for every level of target speed. Figure S2.5 depicts the two pair-wise comparisons for a target speed of 2°/s that were credibly different from zero. In the 5°/s condition, no pair-wise comparisons were credibly different from zero.

Discussion

As in Experiment 1, the results clearly deviate with those expected based on simulations of the degree of retinotopic neural adaptation for each condition. Experiment 1 also revealed a clear effect whereby the most effective mask was one in which the speeds were matched to those of the target. Using two new speeds in Experiment 2, we did find some additional evidence for the importance of the match between the speed of the mask and the stimulus, in that there was a significant interaction between the effectiveness of the different masking conditions across the two target speed conditions. Indeed for a target

moving at 2°/s, the peak of the distribution of thresholds shifted more towards 2°/s compared to the results of Experiment 1. For a target moving at 5°/s, the results were less clear, in fact there were no credible differences between the masking speeds with a target moving at 5°/s. This is possibly due to the fact that the conditions were equated for distance covered over the display and not for presentation time. That is, the target stimuli crossed the same distance over the screen independent of the speed at which they moved, and thus stimuli in the 5°/s target condition are presented for a shorter time duration. It is possible therefore that, the shorter presentation time in the 5°/s condition renders it harder for the visual system to encode the speed of the target, and for this to then have any impact on the speeds used in the mask.

GENERAL DISCUSSION

In this study, we started from the observation that a MMM provided more effective suppression of a moving stimulus than a regular randomly updating CFS mask. This finding did not seem to readily follow from the current assumptions regarding why CFS is an effective suppression paradigm. The robust nature of CFS suppression is generally considered to be the result of the transient nature of the mask, reducing the amount neural adaptation during the interocular competition process at retinotopic stages of the visual system (which have often been implicated in the competition process, Alais, 2012; Blake, 1989). The continuously moving masks we employed yielded a higher degree of simulated retinotopic neural adaptation than the regular CFS mask and therefore should have been less effective (particularly for the slower motion speeds).

In Experiment 1, the speed of individual mask elements of the MMM was manipulated. The influence of varying this speed on masking a moving stimulus was tested and compared to traditional CFS. The data showed an effect of mask speed on the contrast thresholds at which the target could be detected. The highest threshold was obtained for the mask speed that matched the speed of 3°/s at which the target stimulus moved. The thresholds decreased as the CFS mask moved either slower or faster. This finding highlights that regular CFS is not always a powerful, readily applied interocular suppression technique. Instead, the findings of Experiment 1 highlight the feature-selective depth of interocular suppression through CFS. That is, when the properties of the mask are more similar to the

suppressed stimulus, suppression is stronger (see Yang & Blake, 2012 for a further discussion of feature-selective depth of suppression).

Secondly, we explicitly contrasted our findings with a simulation of the degree of retinotopically specific neural adaptation. If transients were critical for CFS to be effective, the mask that contained most feature changes over time was expected to be the most effective. According to the adaptation measure we computed, the masks that showed the least adaptation were the regular CFS, and the moving masks with the highest speeds. However, as shown in Figure 2.2, these were not observed to be the most effective with respect to suppressing the target stimulus ruling out an explanation of the effectiveness of CFS in terms of a simple approximation of retinotopic neural adaptation.

In Experiment 2, we manipulated the speed of the target stimulus and found that the distribution of thresholds changed when the speed of the target stimulus was changed. The results from the second experiment provided some additional support for the importance of matching between the stimulus and the mask, though this interaction was not clear in the 5°/s condition. More critically to our current goal, the results from Experiment 2 again did not agree with the predictions of what would be the most effective mask based on our simulations of retinotopic adaptation.

Although the predictions derived from the simulation of retinotopic adaptation did not agree with the data obtained in both experiments, we should note explicitly that we are not claiming that we have derived predictions from a complete or full-blown model of retinotopic adaptation. Our implementation aimed specifically at extracting edges at different locations in our CFS images and then applying an adaptation process depending on the orientation and contrast polarity of these edges, akin to what a primary visual cortex complex cell might be doing. Thus, in our simulations we did not consider varying spatial frequencies nor differential response properties for magno- and parvocellular pathways. Further, with respect to the adaptation process, we only used one timescale for adaptation and one for recovery (based on neurophysiological measurements), whereas adaptation on multiple timescales or different timescales for different features might be possible. Our simulation, therefore, should only be interpreted as a coarse approximation of retinotopic adaptation. Yet we think this operationalization captures the essence of the concept of

retinotopic adaptation which has been proposed as being important for the effectiveness of CFS.

Methodologically, our results highlight that one should consider using MMM instead of a traditional CFS mask in some contexts to achieve desirable suppression strength. Indeed, our most consistent finding was that a MMM, and especially one that contained motion features similar to the suppressed stimulus was more effective than a traditional CFS mask, highlighting the importance of binocular feature matching (Maehara et al., 2009; Yang & Blake, 2012). Thus, our results suggest that researchers wanting to suppress moving stimuli should also focus on developing MMMs.

Theoretically, our results highlight that the transient nature of the mask is not always the most important aspect of CFS, in the sense that the more spatially transient the mask is, the more effective suppression will be. The initial innovation in developing CFS was exactly the introduction of a transient in one eye which indeed seems crucial for the increase in suppression strength (Tsuchiya et al., 2006). However, the relationship between mask transients and effective suppression does not seem to be as simple as one might assume based on retinotopically specific neural adaptation. Indeed, to achieve reliable suppression through CFS one has to consider the feature similarity between mask and target. This reconnects our understanding of CFS with observations from the binocular rivalry literature in which the importance of feature similarity of competing stimuli has repeatedly been shown (Alais & Melcher, 2007; Alais & Parker, 2006; Stuit et al., 2009).

Whilst the current results challenge the idea that the effectiveness of CFS can be predicted based on a reduction in retinotopically specific neural adaptation, they do not imply that no adaptation-based processes underlie the effect. Indeed, these could potentially be explained by an adaptation mechanism acting at the level of motion speed, for example. That is, given that the visual system can adapt to motion speed (Krekelberg, Boynton, & van Wezel, 2006; Krekelberg, van Wezel, & Albright, 2006), the condition in which mask speed and target speed overlapped would increase the level of adaptation to that specific speed and potentially increase the thresholds for the detection of that speed consistent with our results.

Alternatively, one could also speculate that a mask moving at 3°/s would activate parts of motion area MT that also would be required to represent the target moving at 3°/s.

This explanation would be more consistent with the idea that interocular competition results from a bottleneck imposed by the selective access to higher level areas. If the target and mask in CFS share more properties, then it is possible that they compete more directly for the same neural resources. The stronger motion signals in the mask could dictate that only the mask stimulus reaches higher areas and therefore stays dominant and increases detection thresholds for the suppressed stimulus.

This second explanation could potentially be related to a broader mechanism implicated in the singleton pop-out literature using visual search. In this literature, target-nontarget similarity has been shown to have an influence on the slope of the search function such that the slope is observed to be higher as the similarity between target and nontarget increases (Duncan & Humphreys, 1989). Indeed, in our experiments we observed that it was increasingly easier for participants to detect the moving stimulus when the similarity between mask and target stimulus decreased.

As is apparent from our experiments, and consistent with the work of Hong and Blake (2009), Maehara et al. (2009), and Yang and Blake (2012), the specific properties of the mask play an important role. Indeed, this is also reflected in a recent attempt to construct a dynamical systems model of CFS (Shimaoka & Kaneko, 2011). This model, which extends a minimal model for binocular rivalry introduced by (Wilson, 2007), includes a feature-selective component in addition to the classical cross-inhibition and self-adaptation components.

Thus, regular CFS does not seem to be a general panacea for suppressing stimuli. Indeed, one has to take into account the similarity between features that can be extracted based on the input to each eye, rather than simply increasing the transients in the mask. This finding could help to account for the (implicit) tendency in the literature for different authors to adapt the CFS mask based on the stimulus they are trying to suppress, presumably by matching more closely the characteristics of the to-be-suppressed stimuli and the mask.

CONCLUSION

In this study, we introduced a MMM that was shown to be more effective in suppressing a moving stimulus than a regular CFS mask. We developed an explicit quantification of the degree of retinotopically specific neural adaptation and used this to

make predictions on the effectiveness of our masks. Our results were not consistent with the predictions based on the approximation of retinotopic neural adaptation, and this questions the assumption that the most effective mask will always reflect the avoidance of neural adaptation due to the transient nature of the CFS mask. We conclude that a regular CFS mask that provides effective suppression for static stimuli is not necessarily suited for suppressing moving stimuli and that in general one has to consider the feature match between mask and suppressed stimulus when attempting to use CFS.

ACKNOWLEDGMENTS

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SUPPLEMENTARY FIGURES

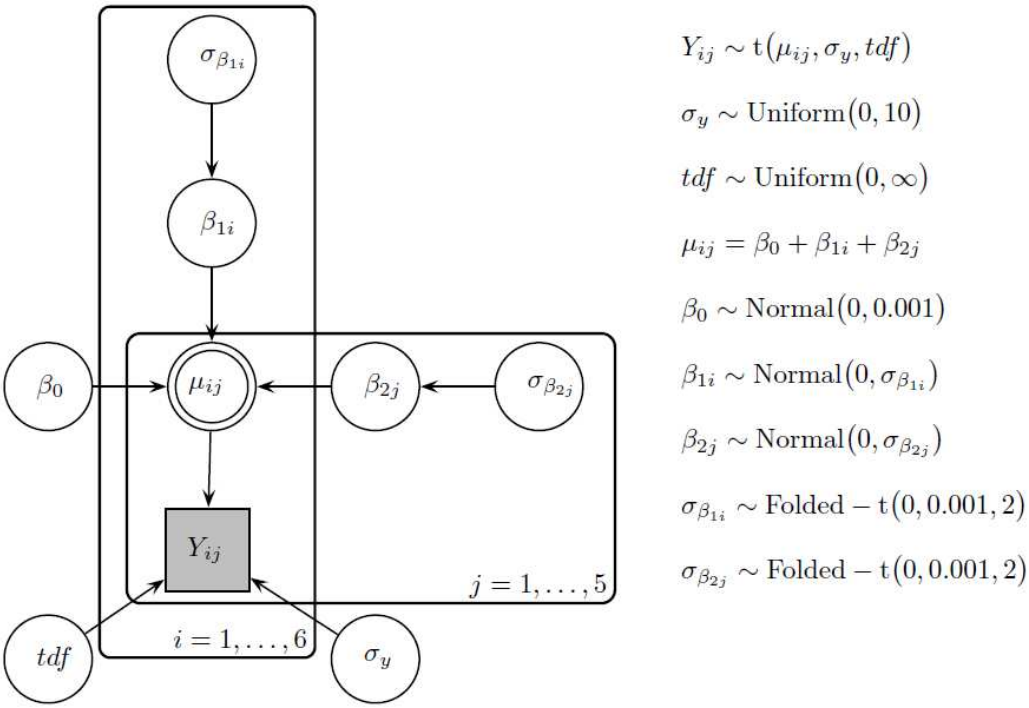

Figure S2.1. Graphical model for the Bayesian version of a one-way repeated measures ANOVA. The data are assumed to come from a t-distribution with a certain mean and standard deviation. The mean is equal to a linear combination of the effect of mask speed (β_{1i}) and a participant-specific effect (β_{2j}).

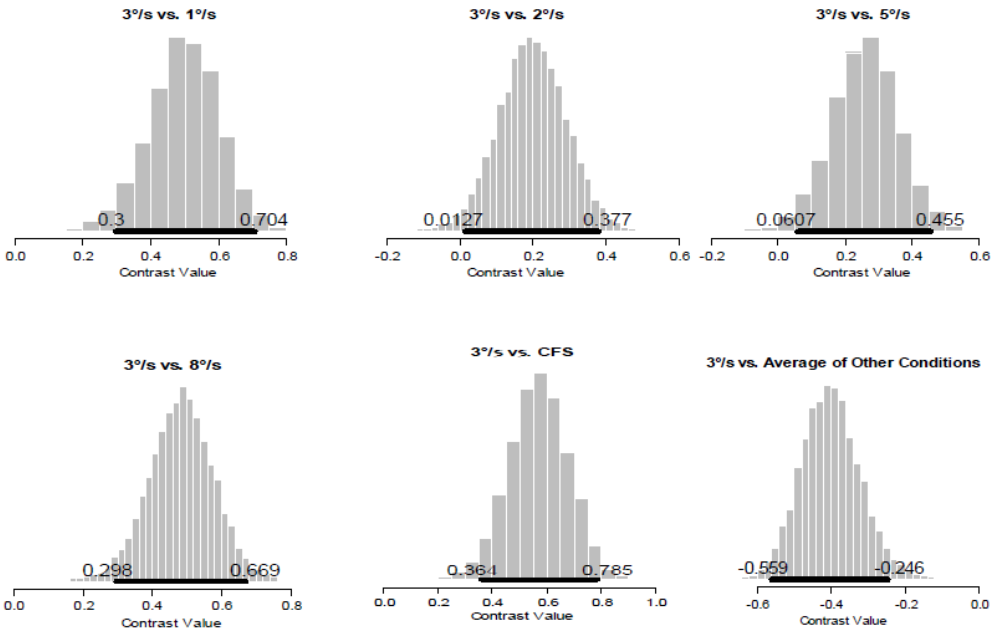

Figure S2.2. Posterior distributions for the pair-wise comparisons between 3°/s and all other levels of mask speed and the regular CFS mask.

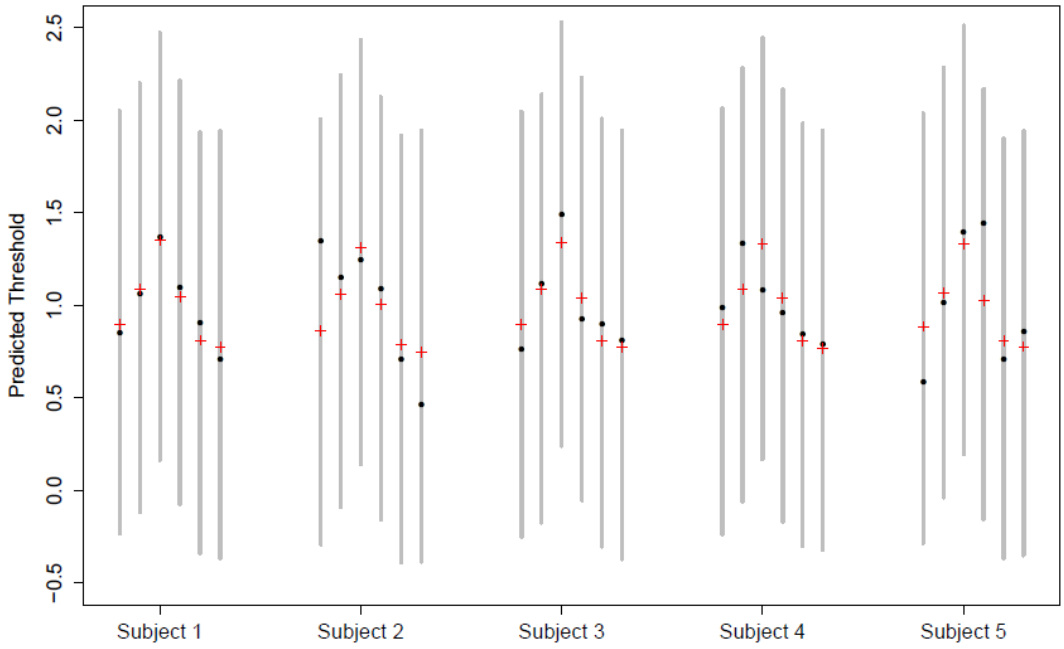

Figure S2.3. Posterior predictive checks for every participant. The red cross is the mean of the predicted values, the gray line the associated 95% HDI and the black dots are the individual data points for every condition. The conditions are ordered as in the bar plots going from a mask moving at 1°/s to regular CFS.

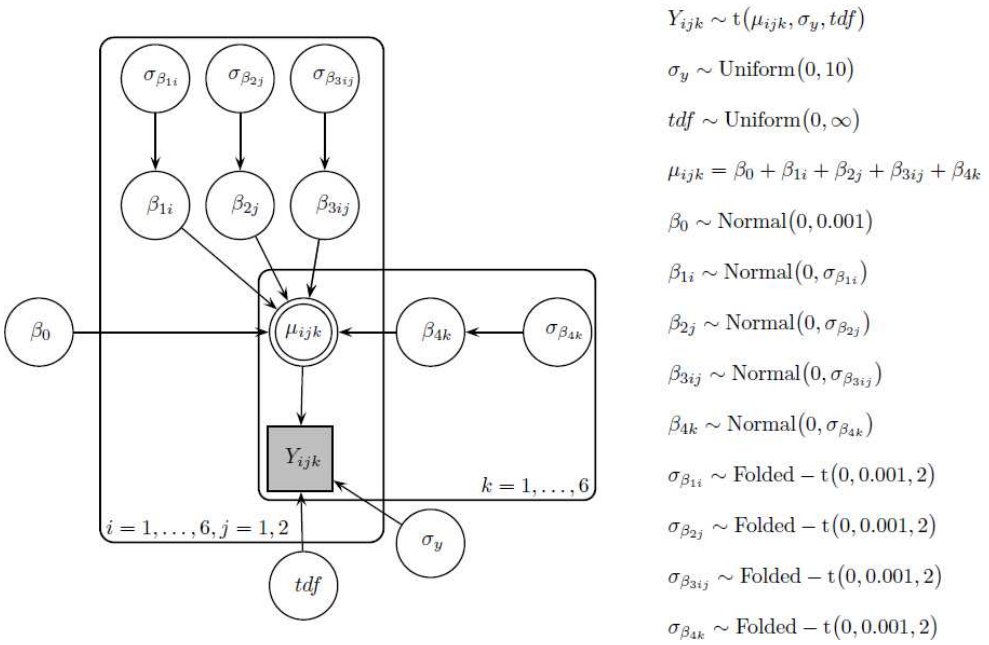

Figure S2.4. Graphical model for the Bayesian version of a two-way repeated measures ANOVA. The data are assumed to come from a distribution with a certain mean and

standard deviation. The mean is equal to a linear combination of the effect of mask speed (β_{1i}), target speed (β_{2j}), their interaction (β_{3ij}) and a participant-specific effect (β_{4k}).

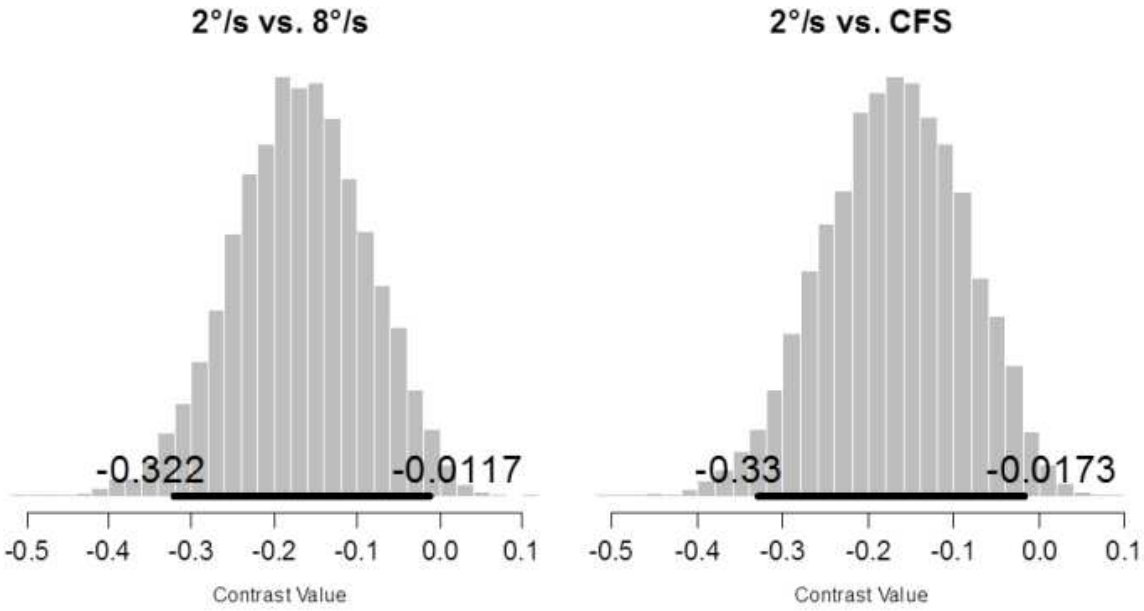

Figure S2.5. Posterior distributions for the pair-wise comparisons between a mask moving at 2°/s and 8°/s and 2°/s and CFS, respectively, for the 2°/s target speed condition.

Chapter 3.
Serial correlations in Continuous Flash Suppression

Research on visual rivalry has demonstrated that consecutive dominance durations are serially dependent, implying that the underlying competition mechanism is not driven by some random process but includes a memory component. Here we asked whether serial dependence is also observed in continuous flash suppression (CFS). We addressed this question by analyzing a large data set of time series of suppression durations obtained in a series of so-called 'breaking CFS' experiments in which the duration of the period is measured until a suppressed target breaks through the CFS mask. Across experimental manipulations, stimuli, and observers, we found that (1) the distribution of breakthrough rates was fit less well by a gamma distribution than in conventional visual rivalry paradigms, (2) the suppression duration on a previous trial influenced the suppression duration on a later trial up to as long as a lag of 8 trials, and (3) the mechanism underlying these serial correlations was predominantly monocular. We conclude that the underlying competition mechanism of CFS also includes a memory component that is primarily, but not necessarily exclusively, monocular in nature. We suggest that the temporal dependency structure of suppression durations in CFS is akin to those observed in binocular rivalry, which might imply that both phenomena tap into similar rather than distinct mechanisms.

Moors, P., Stein, T., Wagemans, J., van Ee, R. (2015). Serial correlations in continuous flash suppression. *Neuroscience of Consciousness*, niv010.

INTRODUCTION

A main challenge for the visual system is to create a stable perceptual world from a noisy stream of sensory input. One way in which perceptual continuity can be achieved is by having the current percept not only be influenced by the current input, but also by the input from the recent past. This would necessarily entail a certain degree of serial dependence in time series pertaining to visual perception. Indeed, recent studies have indicated that perceived orientation, numerosity, or face identity are influenced by stimuli presented in the recent past (Cicchini, Anobile, & Burr, 2014; Fischer & Whitney, 2014; Liberman, Fischer, & Whitney, 2014).

The question of serial (in)dependence in time series has also been addressed in studies on visual rivalry where perception alternates between competing interpretations of the sensory input in a seemingly random fashion (Alais, 2012; Alais & Blake, 2015; Blake & Logothetis, 2002). Early studies on visual rivalry reported that consecutive percept durations did not show any relationship, and if they did, the correlation was judged to be too small to be meaningful (Blake, Fox, & McIntyre, 1971; Borsellino, De Marco, Allazetta, Rinesi, & Bartolini, 1972; Fox & Herrmann, 1967; Lehky, 1995; Logothetis, Leopold, & Sheinberg, 1996; Walker, 1975). More recently, however, a number of studies have rejected the independence between successive dominance durations in visual rivalry (Mamassian & Goutcher, 2005; Pastukhov & Braun, 2011; van Ee, 2005, 2009). Small, but consistently significant serial correlations (most pronounced at lag 1) have been reported for both binocular rivalry as well as for the ambiguously rotating sphere (van Ee, 2009). The finding that consecutive percept durations are serially dependent is important because it provides a footprint of the neural alternation mechanism (van Ee, 2009). That is, it shows that the underlying dynamics are not completely random but include a memory component (as revealed by the serial correlations).

In this paper, we focus on continuous flash suppression (Tsuchiya & Koch, 2005; Tsuchiya, Koch, Gilroy, & Blake, 2006), an interocular suppression paradigm in which discrepant images are presented to corresponding retinal locations of both eyes. In CFS, the input to one of the eyes is continuously updated at a rate of about 100 ms (i.e., ~10 Hz) yielding prolonged and stable suppression of the stimulus presented to the other eye. Although it is still debated whether CFS is simply just a stronger form of binocular rivalry or

involves distinct mechanisms (Kaunitz, Fracasso, Skujevskis, & Melcher, 2014; P. Moors, Wagemans, & de-Wit, 2014; Tsuchiya & Koch, 2005; Tsuchiya et al., 2006), the main factors involved in visual rivalry in general, cross-inhibition and self-adaptation, presumably also come into play during CFS (Shimaoka & Kaneko, 2011). As serial correlations could provide a footprint of the underlying alternation mechanism (van Ee, 2009), a first goal of this paper was to analyze the pattern of serial correlations in CFS by capitalizing on a large data set (n = 393 sessions) consisting of suppression durations obtained in several different so-called breaking CFS (b-CFS) experiments (Jiang et al., 2007; Stein, Hebart, et al., 2011). b-CFS refers to a paradigm in which CFS has been implemented to study unconscious visual processing[1]. In a typical b-CFS study, the CFS mask and a target stimulus of interest are presented to different eyes on each trial. The target stimulus will initially be suppressed from visual awareness but will eventually, after several seconds, "break through" the CFS mask (i.e., become detectable). In these experiments, suppression duration is the main dependent variable and is used as a measure to assess whether different classes of stimuli break suppression differentially. For example, the classic study by Jiang et al. (2007) showed that mean suppression durations for upright faces are shorter than those for inverted faces.

A second goal of this paper pertains to a longstanding debate in the literature on binocular rivalry with respect to the nature and site of interocular suppression. That is, does binocular rivalry suppression entail inhibitory interactions between neurons at a monocular level (Blake, 1989; Levelt, 1965; Verhoeff, 1935) or does competition also occur at levels upstream in the visual cortex, involving competition between binocular neurons (Logothetis et al., 1996; Walker, 1978)? Although this debate has been settled more or less by proposing a hybrid view of binocular rivalry in which rivalry is proposed to happen at multiple stages in the visual hierarchy, both at the monocular and binocular level (Blake & Logothetis, 2002; Tong et al., 2006), the nature of our data set enabled us to shed some more light on this issue. That is, our data set contains two different types of b-CFS experiments, one in which the eye to which the CFS mask was presented was determined randomly on each trial (*variable eye presentation*) and the other in which the eye to which the CFS mask is presented was kept fixed throughout the experiment (*fixed eye presentation*). This enabled us to test the extent to which potential serial correlations in CFS are driven by monocular rather than binocular

[1] Note that the validity of using b-CFS to infer unconscious visual processing has been questioned (Stein, Hebart, & Sterzer, 2011; Stein & Sterzer, 2014), yet a discussion of this issue is beyond the scope of this paper.

mechanisms. Furthermore, it is currently still debated whether differences in suppression times are mostly driven by low-level rather than high-level mechanisms (Gayet et al., 2014; Hesselmann & Moors, 2015; Lupyan & Ward, 2013; Pinto, van Gaal, de Lange, Lamme, & Seth, 2015). The extent to which potential serial correlations are predominantly relying on monocular or binocular mechanisms could also shed some light on this discussion.

In the remainder of this paper, we start by describing the data set that was used for the analysis. In the first part of the analysis, we summarize the data set through a classical analysis of fitting a gamma distribution to the breakthrough rate distribution. In the second part, we report on serial correlations of suppression durations observed across experiments with different observers, target stimuli, and CFS masks.

MATERIALS AND METHODS

The data set

Our data set consists of 24 different experiments ran in four separate studies (see Tables 3.1 and 3.2). Three of these studies (16 experiments) have already been published (Heyman & Moors, 2014; Stein, Seymour, Hebart, & Sterzer, 2014; Stein, Sterzer, & Peelen, 2012). We refer to these studies for the methodological and procedural details of each experiment. All reported studies were conducted in line with the ethical principles regarding research with human participants as specified in The Code of Ethics of the World Medical Association (Declaration of Helsinki). The study was approved by the respective local ethics committees (Ethical Committee of the Faculty of Psychology and Educational Sciences (EC FPPW) of the University of Leuven, and the Charité ethics committee), and all participants gave written informed consent before starting the experiment. Note that Experiment 2 reported in Heyman and Moors (2014) consisted of a test-retest design in which the same experiment was run on the same set of subjects on two consecutive days. Given that these sessions were run on separate days, these two experimental sessions are regarded as two experiments in our analysis. Note, however, that including only one of the sessions rather than both did not change the results. The remaining experiments comprise hitherto unpublished data. All unpublished PM experiments involved presenting illusory shape stimuli in a typical b-CFS design (for a partial report of these data, we refer to Moors, van Crombruggen, Wagemans, van Ee, & de-Wit, 2013). TS16 comprises an unpublished control

experiment belonging to the set of experiments reported in Stein et al. (2014). TS4 and TS5 refer to unpublished data sets in the context of Stein et al. (2012). The number of trials in all these experiments ranged between 192 and 768. We aimed at including only experiments that contained ~200 trials at least since this yields ~90% power to detect a correlation of ~0.2 (van Ee, 2009).

As mentioned in the Introduction, our data set contains two different types of b-CFS experiments, depending on whether the CFS mask was presented in the same eye throughout the experiment or randomly to one of both eyes on each trial. We refer to these experiments as fixed eye (n = 290) and variable eye experiments (n = 103), respectively. A summary of some experimental details for both data sets can be found in Tables 3.1 and 3.2.

All experiments consisted of the typical b-CFS design. A CFS mask (with varying properties, see Tables) was presented to the dominant or non-dominant eye or variably to one of both eyes and the suppressed stimulus was presented to the other eye and gradually increased in contrast. Additionally, in all TS experiments the CFS mask was gradually decreased in contrast throughout a trial, to ensure sufficient breakthroughs for all participants (E. Yang, Zald, & Blake, 2007) (none of the PM experiments relied on this procedure). The specifics of this mask fade-out procedure are reported in Tables 3.1 and 3.2. It is important to note that this mask fade-out procedure has important implications for the resulting suppression durations. Given that the mask invariably disappears after a fixed presentation time, the suppression duration distribution is necessarily censored at this point. Upon breakthrough, participants always had to perform a localization task (i.e., indicate whether the stimulus was presented above or below fixation or whether it was presented left or right of fixation, or in which quadrant the stimulus was shown) and the time it took participants to make the localization response was recorded as the suppression duration. Because blocking rather than randomizing experimental conditions could artificially induce serial correlations, only experiments in which all experimental conditions were randomized across trials were included in the data set.

Table 3.1. Description of data set for *fixed eye* CFS experiments.

Experiment	Participants	Number of Trials	Stimulus Fade-in Time (s)	Mask Fade-Out Onset (s)	Mask Fade-Out Time (s)	Max trial duration (s)	Mask type	Published as
PM1	19	300	2	/	/	/	100 squares between 1° and 2°	Moors, van Crombruggen, Wagemans, van Ee, & de-Wit (2013)
PM2	20	192	2	/	/	/	144 geometrical shapes	Moors et al. (2013)
PM3	20	288	2	/	/	/	144 geometrical shapes	Unpublished
PM4	18	308	2	/	/	/	200 squares between 0.2° and 1.2°	Heyman & Moors (2014)

PM5	31	460	2	/	/	/	200 squares between 0.2° and 1.2°	Heyman & Moors (2014)
PM6	31	460	2	/	/	/	200 squares between 0.2° and 1.2°	Heyman & Moors (2014)
PM7	21	288	2	/	/	/	144 geometrical shapes	Unpublished
PM8	20	288	2	/	/	/	48 geometrical shapes	Unpublished
TS9	12	576	1.1	1.1	4	7	Circles between 0.3° and 1.4°	Stein, Seymour, Hebart, & Sterzer (2014) – Exp 1a
TS10	16	384	1.1	1.1	4	7	Circles between 0.3° and 1.4°	Stein et al. (2014) – Exp 1b
TS11	16	384	1.1	1.1	7	10	Circles between 0.3° and 1.4°	Stein et al. (2014) – Exp 1c

TS12	12	384	1.1	1.1	4	7	Circles between 0.3° and 1.4°	Stein et al. (2014) – Control Exp 1a
TS13	14	768[2]	1.1	1.1	4	7	Circles between 0.3° and 1.4°	Stein et al. (2014) – Control Exp 1b
TS14	12	256[3]	1.1	1.1	7	10	Circles between 0.3° and 1.4°	Stein et al. (2014) – Control Exp 2
TS15	12	256	1.11	1.1	7	10	Circles between 0.3° and 1.4°	Stein et al. (2014) – Control Exp 3
TS16	16	384[4]	1.1	1.1	7	10	Circles between 0.3° and 1.4°	Unpublished

[2] The experiment crashed for 1 participant and only 524 trials were recorded for this participant.
[3] The experiment crashed for 2 participant and only 252 and 240 trials were recorded for these participants.
[4] The experiment crashed for 1 participant and only 260 trials were recorded fort his participant.

Table 3.2. Description of data set for *variable eye* CFS experiments.

Experiment	Participants	Number of Trials	Stimulus Fade-in Time (s)	Mask Fade-out Onset (s)	Mask Fade-out Time (s)	Maximum Trial Duration (s)	Mask type	Published as
TS1	12	288	1	1.1	7	10	Circles between 0.4° and 1.8°	Stein, Sterzer, & Peelen (2012) – Exp 2
TS2	11	320	1	1.1	7	10	Circles between 0.4° and 1.8°	Stein et al. (2012) – Exp 4
TS3	10	200	1	1.1	7	10	Circles between 0.4° and 1.8°	Stein et al. (2012) – Exp 5
TS4	10	320	/1	1.1	7	10	/	Unpublished
TS5	21	240	/1	1.1	7	10	/	Unpublished
TS6	13	192	1	1	7	10	Circles between 0.4° and 1.8°	Stein, Peelen, & Sterzer (2011) – Exp 1
TS7	13	192	1	1	7	10	Circles	Stein et al. (2011) –

							between 0.4° and 1.8°	Exp 2
TS8	13	192	1	1	7	10	Circles between 0.4° and 1.8°	Stein et al. (2011) – Exp 3

Data analysis

The first part of the analysis consisted of cleaning the data in two steps to ensure no correlations would be observed that could be attributable to either of the following two factors. First, we excluded for each observer the first 5 trials of each session (i.e., akin to the removal of the first 30 seconds of each trial in van Ee, 2009). Second, we corrected the data for drift (i.e., suppression times tend to become shorter, on average, over the course of the experiment). Because the drift was potentially non-linear, we performed a local regression (LOESS) on the data set, with trial number as the predictor of suppression duration. The smoothing parameter was automatically selected based on the bias-corrected Akaike information criterion (Hurvich, Simonoff, & Tsai, 1998).

As highlighted in the description of the data set, all TS experiments relied on a mask fade-out procedure, in which the CFS mask contrast was gradually ramped down over the course of a trial. Because this introduces an artificial cut-off in the suppression duration distributions (i.e., when the CFS mask disappears, participants will always see the stimulus in the following second or so), we removed all trials in which the recorded suppression duration was higher than the time point at which the CFS mask disappeared. If this led to a removal of more than 10% of trials, we removed this participant from the data set. We used this cut-off to ensure that potential serial correlations could not be induced by responses to stimuli in the absence of a CFS mask. This procedure led to a removal of 12 and 24 participants for the fixed and variable eye experiments, respectively. Please note however that the overall pattern of serial correlations (especially with respect to the early lags) does not change when these participants are included (see Supplementary Files for a figure including these participants, as well as excluding the cleaning steps).

Because the suppression durations follow a non-normal distribution, we calculated Spearman rank correlations at the various lags, where lag n refers to the n^{th} trial before the current trial. The maximum lag that was considered was lag 10 because this proved to be the lag up to which the data were most reliable for most participants. A Fisher-z transformation was applied to the Spearman correlations before averaging them (due to the range of trials in the experiments included, a weighted average was used, where the weight was the number of trials used in the experiment). After averaging, the resulting correlation was back-transformed using the inverse transformation. The significance of the average Spearman

correlations was assessed using one-sample randomization tests in Fisher-z space. Since this analysis essentially involves comparing 10 different *p*-values against zero, the significance of the *p*-values was determined by controlling the false discovery rate (FDR, at 5%), using the method introduced by Benjamini and Hochberg (1995). A summary of the analysis pipeline is visualized in Figure 3.1. Additionally, to compare both experiment types, we used polynomial mixed-effect regression modelling with random intercepts and random slopes for participants (without correlations between random effects). Polynomial regression was used to account for the nonlinear relationship between lag and the observed Spearman correlation (infra). Drop-in-deviance tests were used to compare different statistical models.

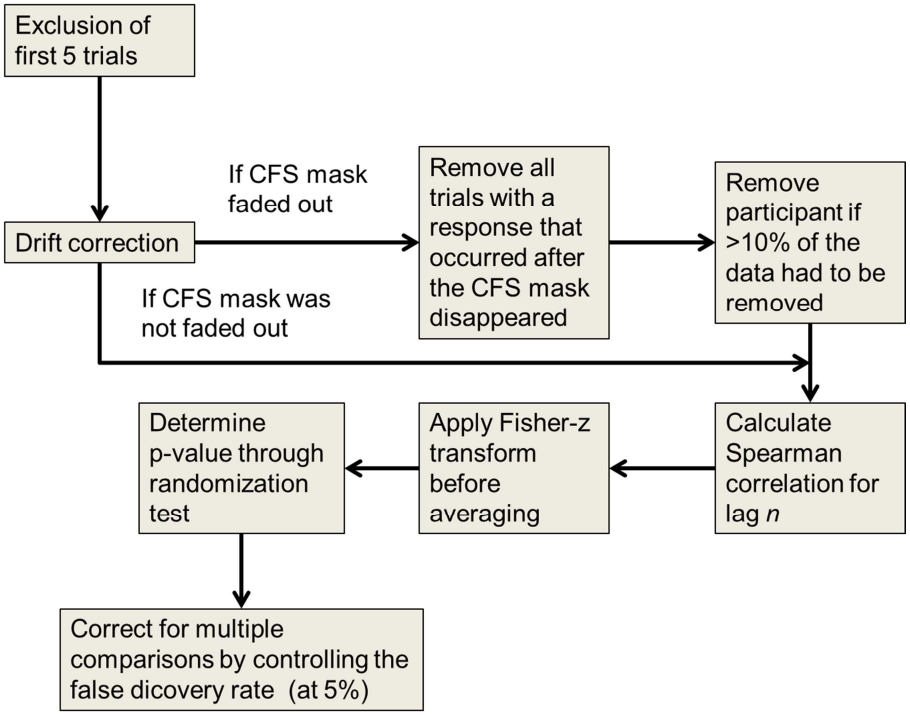

Figure 3.1. An overview of the analysis pipeline.

RESULTS

Suppression duration variability across observers

We first provide a description of the data set in terms of a classic analysis of suppression durations. Because Brascamp et al. (2005) showed better fits for alternation rates rather than percept duration for a range of visual rivalry stimuli, we fitted a gamma distribution to the distribution of breakthrough rates rather than suppression durations (i.e., breakthrough rate is 1/suppression duration). Figure 3.2 depicts the distributions of two

different observers overlaid with the best fitting gamma distributions (see Supplementary Figures S3.1, S3.2, and S3.3 for an overview of all observers, split up by eye for the variable eye experiments, for the fixed and variable eye experiments, respectively). For both experiment types, scatter plots of the shape and scale parameters of the individual observer fits are depicted. Note that there is considerable inter-individual variability in the estimates of the shape and scale parameters, reflecting the variability in breakthrough rates across observers. Furthermore, it should be noted that the tight negative relationship between shape and scale parameters in log-log space is to be expected due to the parameterization of the gamma distribution (Borsellino et al., 1972; Brouwer & van Ee, 2006; van Ee, Noest, Brascamp, & van den Berg, 2006; Wagenmakers & Brown, 2007). The boxplots summarize the goodness of fit quantified through the probability *pKS* obtained from the Kolmogorov-Smirnov test, which involves the largest overall deviation between the empirical and fitted cumulative distribution (Brascamp et al., 2005) (referred to as the D statistic). *pKS* ranges between 0 and 1 where higher values indicate good fit. Because estimated parameters were used for the Kolmogorov-Smirnov test, the associated *pKS* value is no longer valid (Durbin, 1973). Therefore, we used a Monte Carlo procedure to compute *pKS*. In this procedure, we generated new data sets for each participant based on the estimated parameters and computed the D statistic for each simulated data set. For each participant, we repeated this procedure 10,000 times and computed *pKS* as the proportion of simulated D values more extreme than the D value observed in the data.

As is apparent from Figure 3.2, the median *pKS* value is equal to .08 (.17 and .11 for the variable eye experiments, right and left eye respectively), indicating that the fit quality is generally low for the breakthrough rate distributions, especially compared to Brascamp et al. Indeed, for both the fixed and variable eye experiments, the upper limit of the interquartile range of the *pKS* never exceeds the lower limit of the interquartile range reported in Brascamp et al. This discrepancy between the quality of fits generally observed in visual rivalry and the ones observed here is further touched upon in the discussion.

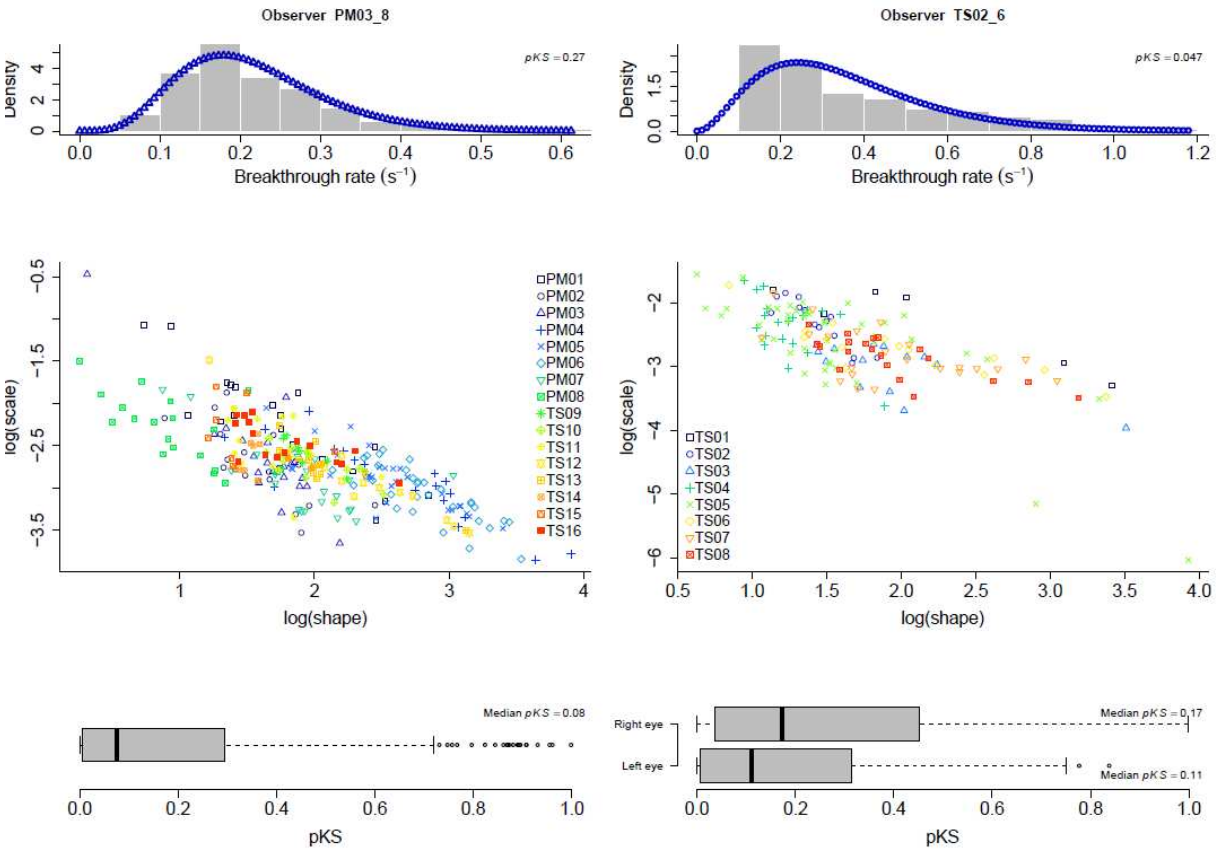

Figure 3.2. Distribution analysis of breakthrough rate. (left) Fixed eye experiments. The histogram depicts the distribution of one typical observer overlaid with the best fitting gamma distribution. The middle left figure depicts the shape and scale estimates for all observers in log-log space. Goodness of fit was quantified through the *pKS* statistic, relying on the Kolmogorov-Smirnov test. A summary is depicted in the bottom left boxplot (high values indicate good fit). (right) Variable eye experiments.

Aggregated serial correlation data

Figure 3.3 depicts the Spearman rank correlation coefficients up to lag 10 aggregated across all observers using a weighted average (weighed by the number of trials used in the experiment) (black line; shaded gray area indicates the bootstrapped 95% confidence interval), for both data sets. To assess the variability across experiments, the aggregated data are also plotted for each experiment separately (colored lines). For the fixed eye experiments, the correlation at lag 1 is positive and significantly different from zero (r = .10). Furthermore, rather than immediately dropping to zero, the serial correlations gradually decay to zero until they are no longer significant from lag 9 onwards. In contrast, for the variable eye experiments, a markedly different pattern arises. The correlation at lag 1 is slightly lower (r =

.09) and quickly drops to zero from lag 3 onwards. This difference between experiment types is confirmed by a polynomial mixed effects regression analysis. A model including main effects of lag (polynomials up to the order of three) and experiment type and their interaction was preferred over a model only including the main effects (drop-in-deviance test, $\chi(3) = 9.4079$, $p = .024$).

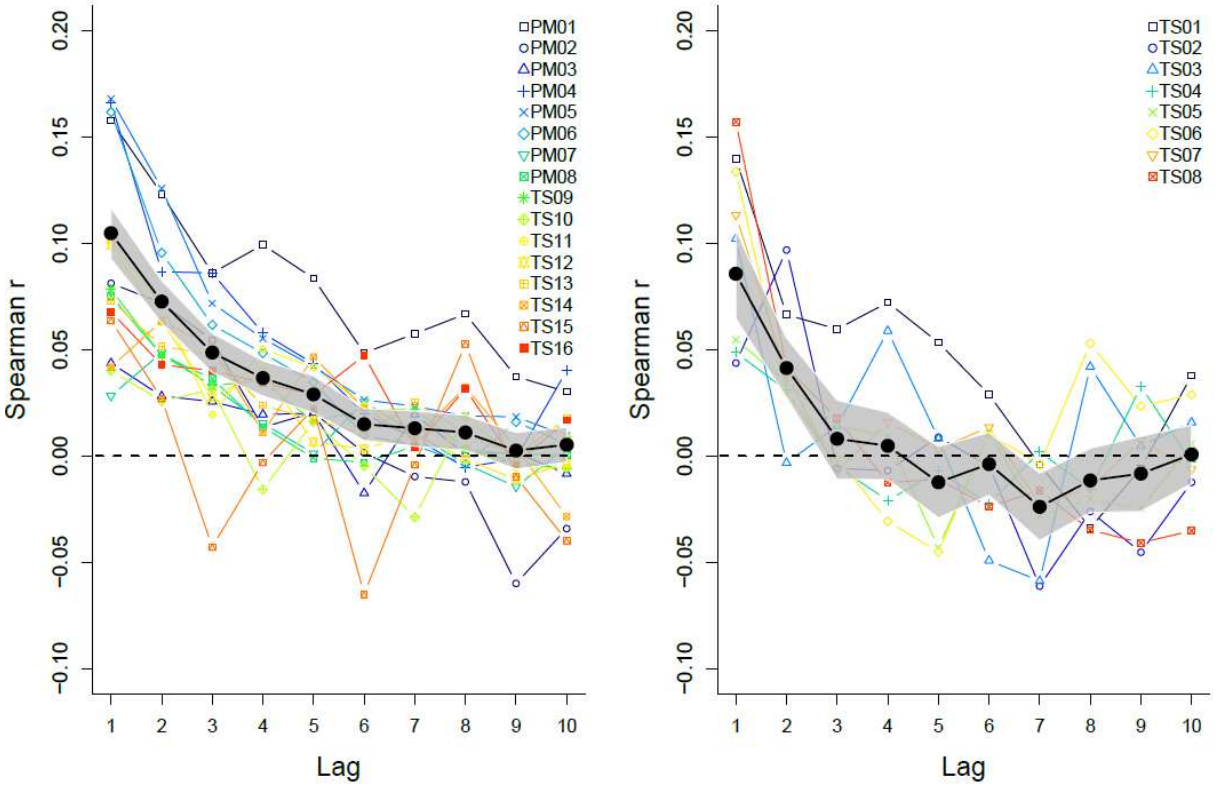

Figure 3.3. Aggregated serial correlation data for the fixed (left) and variable (right) eye experiments. Mean Spearman rank correlations across all observers as a function of lag. The gray, shaded area indicates the 95% bootstrapped confidence intervals. The colored lines depict the mean Spearman rank correlations for each experiment separately.

The variable eye experiments contain both trials in which the CFS mask is kept constant on trial *n + 1* as well as switched to the previously suppressed eye. Thus, we were interested in examining the influence of swapping eyes across trials for these experiments. Therefore, we split the data for each observer into a data set in which for all trials the CFS mask was presented to the same or different eye on the previous trial (i.e., lag 1). We restricted our analyses to lag 1 only to ensure that we still had sufficient data. Figure 3.4 depicts the results of this analysis. As is apparent from this figure, the major contribution to

the positive lag 1 correlation observed for the variable eye experiments stems from the trials in which the CFS mask is presented to the same eye as in the previous trial. Moreover, rather than being positive, the lag 1 correlation for the swap trials is low and negative, presumably due to the participants' eye dominance.

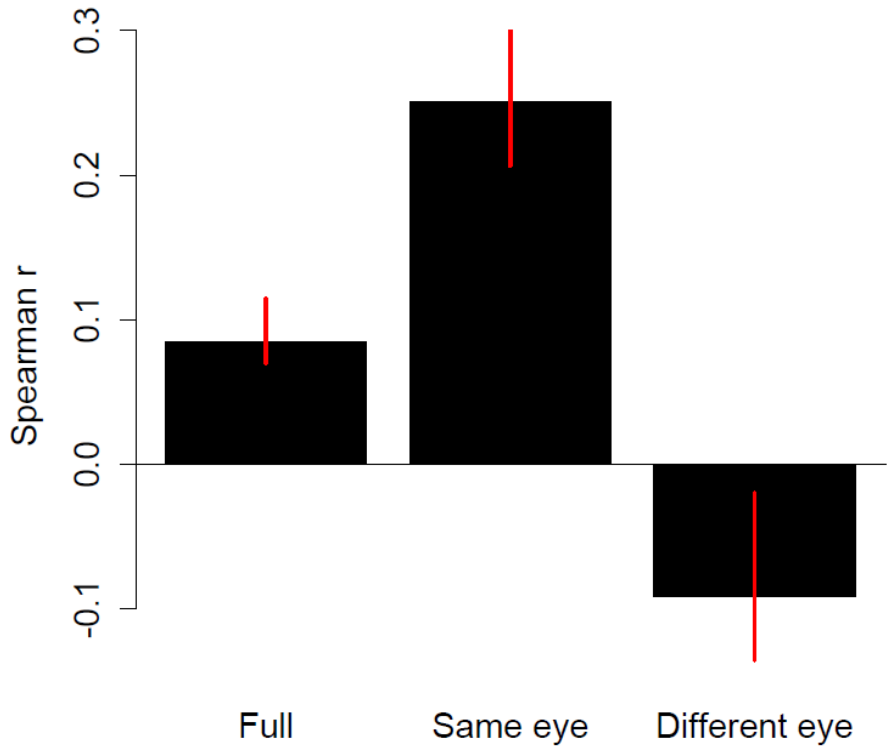

Figure 3.4. Lag 1 correlation analysis for the variable eye experiments. The bar plot depicts the lag 1 Spearman correlations for the full data set (as depicted in Figure 3.3), and the data set split up in trials in which the CFS mask was always presented to the same or different eye on the previous trial. The same eye trials contribute significantly to the positive lag 1 correlations. Error bars denote 95% confidence intervals.

Control data

To ensure that our results were specific to CFS and not any reaction-time based process in general, we subjected the data of the control experiments reported in Stein, Hebart, et al. (2011) (Experiment 1) and Stein, Peelen, and Sterzer (2011) to the same analysis as we did above. Their particular experiments tested whether upright faces (or face-like stimuli) would break suppression faster than inverted faces in a control condition in which the target and mask stimulus were both presented in both eyes. Because the inclusion criteria

for these experiments were quite stringent (same fade-in time and onset as in the CFS condition, CFS and control condition blocked rather than mixed), these experiments contained less trials than those in the main data set (n = 120). Figure 3.5 depicts the aggregated serial correlation data for these two experiments. As is apparent from this figure, no consistent serial correlation pattern was observed. Indeed, a mixed-effects regression model with a main effect of lag was not preferred over one not including the main effect ($\chi(1) = 2.42$, $p = .1197$)

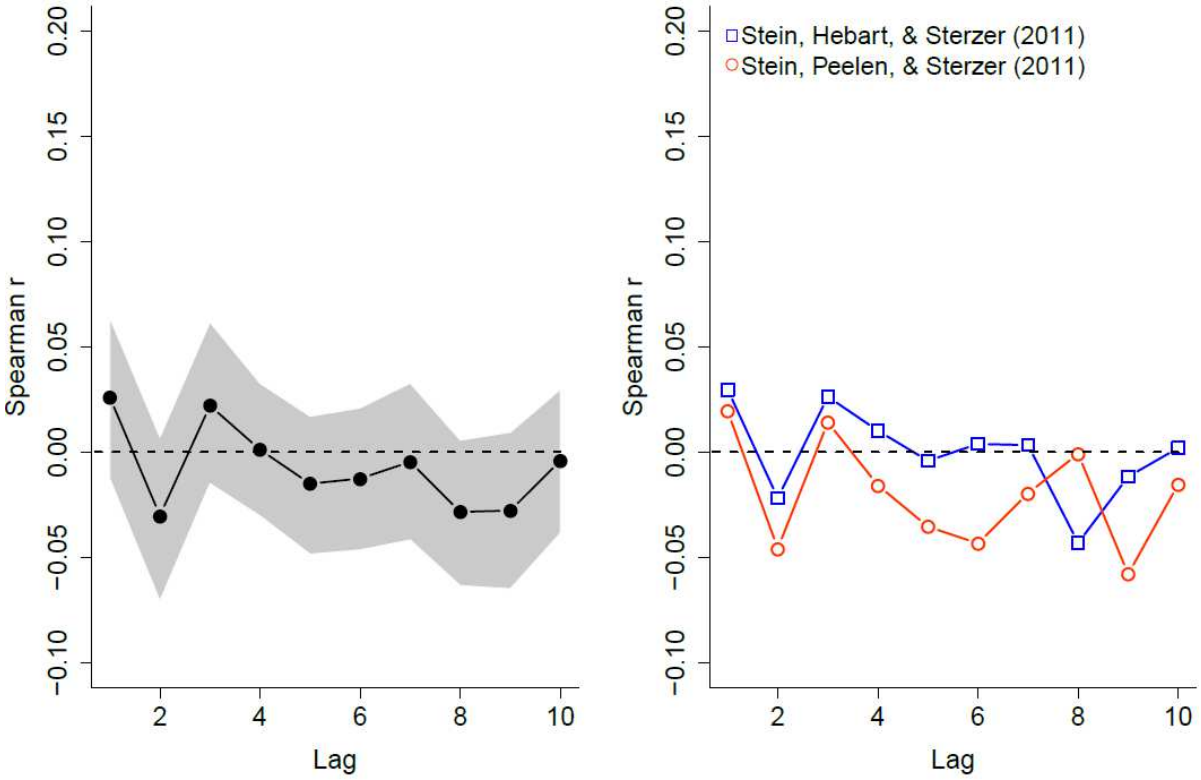

Figure 3.5. Serial correlations for the control data set. For the binocular control condition (i.e., the CFS mask and the target stimulus are both presented to both eyes), there is no consistent decay in serial correlations.

DISCUSSION

The goal of this study was to analyze whether successive suppression durations obtained in a b-CFS experiment show serial dependence, as this is considered to be a marker of the underlying neural alternation mechanism. When serial correlations are observed, this indicates that the underlying mechanism is not random and includes a memory component (van Ee, 2009). We have performed a serial correlation analysis on a large data set of b-CFS sessions. In line with previous reports on binocular and perceptual rivalry, we observed

small, but significant serial correlations in the suppression durations obtained in several, different b-CFS experiments. It should be noted that none of these experiments was originally designed to test the temporal dependency structure of suppression durations in a b-CFS paradigm. Therefore, one might argue that the results we obtained are merely due to methodological peculiarities inherent to some of the experiments we included. In contrast, we consider the diversity of our data set as a strength and are encouraged by the fact that similar temporal dependency patterns (taking into account sampling variability) are observed across observers, stimuli, and laboratories.

The observations reported in this study further substantiate a growing literature on serial dependence in visual rivalry and furthermore provides insight into the temporal dynamics of interocular suppression induced through continuous flash suppression (CFS). It has been debated whether CFS relies on distinct mechanisms or operates similar to binocular rivalry (L. N. Kaunitz et al., 2014; P. Moors et al., 2014; Tsuchiya & Koch, 2005; Tsuchiya et al., 2006). Our results suggest that the temporal dependency structure of suppression durations in CFS is akin to those observed in binocular rivalry which might imply that both phenomena tap into similar rather than distinct mechanisms. Interestingly, our distributional analysis of breakthrough rates indicated that fitting a gamma distribution to breakthrough rates yielded considerably worse fits (as quantified through the probability *pKS*) compared to what has been observed in other studies (Brascamp et al., 2005). This should not be too surprising, however, given that CFS is known to substantially increase the proportion of long suppression durations. On top of the positively skewed distribution that is generally observed, this aspect introduces a long and thick tail in the distribution that is not well captured by a gamma distribution. Moreover, the mask fade-out procedure that was employed in some of the experiments introduced, for some observers, a second peak in the distribution when the CFS mask reached a low contrast. Nevertheless, it should be noted that the overall low fit quality was also observed in the experiments that did not rely on this procedure (i.e., all PM experiments, see Table 3.1). When comparing both experiment types, however, the variable eye experiments yielded somewhat better fits compared to the fixed type experiments. This might be explained by the fact that these experiments all relied on a mask fade-out procedure, which might have facilitated breakthroughs for some observers and yielded a better fit compared to the absence of a mask fade-out procedure (despite the

censoring of the breakthrough rate distribution). In sum, it remains to be investigated whether the distribution of suppression durations or breakthrough rates can be captured by a single distribution or rather that a mixture of different distributions is more suitable to take into account the very long suppression durations observed in a typical b-CFS experiment.

Previous studies have generally shown evidence for serial dependence in dominance durations, mostly restricted to lag 1. Interestingly, we observed a gradual decay of serial correlations in the fixed eye experiments. In the variable eye experiments, the pattern of serial correlations was more in line with a previous study from visual rivalry (van Ee, 2009) in that they were most pronounced at lags 1 and 2 and fell off quickly to zero for longer lags. The same study simulated significant serial correlations at lag 1 using a computational model of visual rivalry (Noest, van Ee, Nijs, & van Wezel, 2007) to which white noise was added at the slow timescale of percept adaptation (van Ee, 2009). The divergence between the observed serial correlation patterns in our fixed and variable eye experiments can also be interpreted in the light of these simulation results. That is, in the fixed eye experiments in which the CFS mask is continuously presented to the same eye, we observed gradually decreasing serial correlations for increasing lags. If the adaptation state of neurons involved in representing the CFS mask is responsible for the serial correlations in suppression durations, one would expect a longer-lasting influence for conditions in which the perceptual dominance of the CFS mask is caused by the continuous presentation to the same eye.

Those simulations, using white noise added at the slow timescale of percept adaptation (van Ee, 2009), did not allow for the reproduction of serial correlations at higher lags. To intuit how higher-order serial correlations (beyond lag 1) can be obtained, the phenomenon of perceptual stabilization for an ambiguous stimulus upon intermittent presentation might be relevant. That is, if an ambiguous stimulus is intermittently presented, it is observed that its perception stabilizes upon repeated presentation (i.e. the percept at repetition $n–1$ transfers to repetition n) (Leopold, Wilke, Maier, & Logothetis, 2002). This phenomenon is generally explained by a short-lived perceptual memory mechanism that favors the most recent percept. A number of studies have shown, however, that perceptual stabilization also involves a longer-term memory mechanism based on the relative proportion of dominance of one or the other stimulus (Brascamp et al., 2008; de Jong, Knapen, & van Ee, 2012). It is possible to model the higher-order serial dependencies by

extending a conventional model in visual rivalry (Noest et al., 2007) to include adaptation at multiple time-scales (Brascamp et al., 2008). More recent work concluded that previously perceived interpretations dominate at the onset of ambiguous sensory information, whereas alternative interpretations dominate prolonged viewing (de Jong et al., 2012). At first instance ambiguous information seems to be judged using familiar percepts, while re-evaluation later on allows for alternative interpretations. Thus, the observed serial dependency structure might be modelled by including adaptation dynamics at multiple time scales (including noise at each of these levels). On a speculative note, the higher-order serial dependency structure observed across all observers might be due to the fact that, during a single trial, CFS invokes mechanisms similar to perceptual stabilization. The continuous updating of the contents of the CFS mask may be thought of as intermittently presenting a stream of visual stimuli that might stabilize the current percept (the CFS mask) and therefore prolong dominance durations compared to regular binocular rivalry.

A second goal of our study pertained to the nature and site of the underlying mechanisms generating the serial dependency in suppression durations. That is, the nature of our data set (fixed versus variable eye experiments) enabled us to test the relative influence of presenting the CFS mask continuously to the same eye rather than randomly swapping it throughout the experiment. As the analysis of the aggregated data indicated, the pattern of serial correlations diverges between the types of experiments considered, fixed versus variable eye presentation of the CFS masks. At first sight this would seem to suggest that both monocular and binocular mechanisms are at play in generating these serial correlations. Furthermore, given the discrepancy between both data sets at lags beyond 2, monocular mechanisms would be primarily responsible for the serial correlations observed at those lags. An additional analysis of the variable eye experiments indicated that the serial correlations observed in these experiments seem to be mostly driven by the trials in which the CFS mask is not switched to the other eye across consecutive trials. Thus, our data suggest that the mechanisms responsible for generating the observed temporal dynamics in CFS are primarily, but not necessarily exclusively, monocular in nature. This observation is well in line with other CFS studies highlighting that adaptation, perceptual learning, and

stimulus-reward learning are primarily monocular (Mastropasqua, Tse, & Turatto, 2015; Seitz et al., 2009; Stein & Sterzer, 2011).

In this respect, it is interesting to note that Logothetis et al. (1996) also observed a lag 1 correlation of ~0.1 for stimulus rivalry. Stimulus rivalry refers to the observation that rapidly and repetitively swapping the rivalling stimuli between the eyes does not substantially change the rivalry dynamics, indicating that rivalry would not be purely eye-based. However, Lee and Blake (1999) have shown that stimulus rivalry is limited to a certain combination of spatiotemporal parameters. Otherwise, eye rivalry dominates. Furthermore, recent studies showed that monocular channels contribute to stimulus rivalry (Brascamp, Sohn, Lee, & Blake, 2013) and that individual differences in the temporal dynamics of conventional binocular rivalry and stimulus rivalry are tightly linked (Patel, Stuit, & Blake, 2014), suggesting that both forms of rivalry might rely on similar mechanisms. In combination with our data, this might indicate that serial correlations in stimulus rivalry also have a monocular basis.

CONCLUSION

In this paper, we asked whether consecutive suppression durations obtained across several different breaking CFS experiments are serially dependent, which would provide evidence for the underlying mechanism being nonrandom and having a memory component. Our serial correlation analysis indicated a gradual decay of serial correlations at the aggregate level for experiments in which the eye to which the CFS mask was presented was kept constant across trials. Thus, we conclude that the underlying competition mechanism of CFS includes a memory component. A different pattern emerged in the experiments in which the eye to which the CFS mask was presented was randomly determined on each trial. Here, serial correlations decayed more rapidly to zero beyond lag 2. However, these correlations at early lags were shown to be due to trials in which the CFS mask was not switched across consecutive trials. This indicates that the observed serial correlations are predominantly driven by a process that is monocular in nature. A control analysis confirmed that the serial correlations were not due to any generic reaction time based process. These findings further substantiate the literature on serial dependence in visual rivalry and furthermore shed light on the similarities and differences between the

underlying dynamics in these breaking CFS experiments and those observed in binocular rivalry. We suggest that the temporal dependency structure of suppression durations in CFS is akin to those observed in binocular rivalry, which might imply that both phenomena tap into similar rather than distinct mechanisms.

ACKNOWLEDGMENTS

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SUPPLEMENTARY FIGURES

Due to the size of the supplementary figures, we have not copied them here. We therefore refer to the published version of the article to consult them.

Chapter 4.
No evidence for surface organization in Kanizsa configurations during continuous flash suppression

Does one need to be aware of a visual stimulus for it to be perceptually organized into a coherent whole? The answer to this question regarding the interplay between Gestalts and visual awareness remains unclear. Using interocular suppression as the paradigm for rendering stimuli invisible, conflicting evidence has been obtained as to whether the traditional Kanizsa surface is constructed during interocular suppression. While Sobel and Blake (2003) and Harris, Schwarzkopf, Song, Bahrami, and Rees (2011) failed to find evidence for this, Wang, Weng, and He (2012) showed that standard configurations of Kanizsa pacmen would break interocular suppression faster than their rotated counterparts. In the current study, we replicate the findings by Wang et al. (2012) but show that neither an account based on the construction of a surface nor one based on the long-range collinearities in the standard Kanizsa configuration stimulus could fully explain the difference in breakthrough times. We discuss these findings in the context of differences in the amplitudes of the Fourier orientation spectra for all stimulus types. Thus, we find no evidence that the integration of separate elements takes place during interocular suppression of Kanizsa stimuli, suggesting that this Gestalt involving figure-ground assignment is not constructed when rendered non-conscious using interocular suppression.

Moors, P., Wagemans, J., van Ee, R., de-Wit, L. (2015). No evidence for surface organization in Kanizsa configurations during continuous flash suppression. *Attention, Perception, & Psychophyics*.

INTRODUCTION

Our phenomenological experience consists of a perceptually organized whole in which the signals sent from the retina have been structured into a meaningful scene composed of distinct objects and surfaces (S. E. Palmer, 1999). This process of perceptual organization occurs in accordance with a set of grouping principles, including proximity, similarity, good continuation, common fate, connectedness, or closure (Wagemans et al., 2012). Given the apparent ease with which perceptual grouping happens, several studies have tested the extent to which it operates independently from attention and/or awareness (Alais & Blake, 2015; Gillebert & Humphreys, 2015; Schwarzkopf & Rees, 2015). Perhaps unsurprisingly, the results of these studies paint a complicated picture, with the results often being contingent on the exact paradigm used to render stimuli non-conscious and with many different perceptual organizational processes being involved. It is probably fair to conclude, however, that there is evidence that some forms of perceptual organization still take place in the absence of attention (Gillebert & Humphreys, 2015; Kimchi, 2009; Moore & Egeth, 1997). Likewise, there is also some evidence that certain forms of perceptual organization still manifest themselves in the absence of awareness (Mitroff & Scholl, 2005; Montoro et al., 2014; Norman et al., 2013).

The question, therefore, becomes what kinds of perceptual organization can occur without attention or awareness, and to what extent does this depend on the paradigm used to render stimuli invisible. In this study, we focus on the Kanizsa stimulus, using a paradigm for suppressing stimuli from awareness that should disrupt higher level processing.

Previous work from our lab has highlighted a potential distinction between perceptual organizational phenomena that involve the grouping of elements in a display and the assignment of figure-ground relationships (Machilsen & Wagemans, 2011; Vancleef et al., 2015; Vancleef, Wagemans, & Humphreys, 2013). Both contour grouping and figure-ground assignment have been shown to contribute to the perception of a surface when presented with the standard Kanizsa stimulus (Conci et al., 2009; Kogo, Strecha, Van Gool, & Wagemans, 2010; Kogo & Wagemans, 2013), yet the contribution of both has been shown to be different with respect to timing and neural correlates (Cox & Maier, 2015; Poort et al., 2012). Indeed, there is evidence that the perceptual organization involved in the construction of a Kanizsa surface is dependent upon the functioning of higher areas in the visual ventral

stream, namely the lateral occipital complex (LOC) (de-Wit, Kentridge, & Milner, 2009; Seghier & Vuilleumier, 2006; Stanley & Rubin, 2003). Other studies have documented activity in areas such as V1 and V2 when perceiving Kanizsa figures, but the latency of these neural responses suggests that feedback processes are involved (T. S. Lee & Nguyen, 2001; von der Heydt, Peterhans, & Baumgartner, 1984). Indeed, a recent TMS study by Wokke, Vandenbroucke, Scholte, and Lamme (2013) suggests that feedback to early visual areas is critical for surface perception in the Kanizsa stimulus. In sum, there is evidence for a distinction between contour grouping and the perception of figure-ground relationships, and that figure-ground perception for the Kanizsa figure in particular relies on processing at higher stages of the ventral stream.

In parallel to this, a paradigm for rendering stimuli invisible, called "continuous flash suppression (CFS)", is known to have a large impact in reducing the flow of visual input to higher visual areas like LOC (Fang & He, 2005; Hesselmann & Malach, 2011). This paradigm renders stimuli invisible by continuously flashing stimuli to one eye, rendering the input to the other eye inaccessible to awareness for extended periods of time (Tsuchiya & Koch, 2005). Given this theoretical background, it would seem logical to predict that it would be impossible for this Gestalt to be constructed or influence behavior when suppressed from awareness using CFS (see also Alais and Blake, 2015), because the flow of activity to higher areas involved in the construction of the Kanizsa stimulus is blocked with CFS. The evidence however, is currently mixed.

In the first study on the perception of Kanizsa figures using CFS, Harris, Schwarzkopf, Song, Bahrami, and Rees (2011) showed that observers were unable to discriminate in which direction a suppressed Kanizsa triangle configuration was pointing, yet could do so perfectly when the stimulus was visible. This is consistent with previous work using binocular rivalry, in which Sobel and Blake (2003) set out to test whether the perception of so-called illusory contours (induced by certain Kanizsa stimuli) had similar effects on initiating switches between eyes compared to contours derived from physically presented contrast edges. When physically presented edges moved over a suppressed stimulus, the suppressed stimulus became visible to the participant, while this did not happen for configurations that elicited a contour percept that did not derive from a

physically presented edge. In a more recent study, however, Wang, Weng, and He (2012) used a variant of the CFS (the breaking-CFS or b-CFS) paradigm to measure how long it took for configurations of standard pacmen (that are able to induce a surface percept when fully visible) to overcome suppression induced by CFS. In apparent contrast with the previous studies, Wang et al. (2012) observed that configurations able to induce a triangular Kanizsa surface break suppression faster than non-surface-inducing control conditions (even when controlling for mirror symmetry in the control stimulus). Wang et al. (2012) suggested that this result provided evidence that the processes involved in constructing the Kanizsa surface are able to manifest even when suppressed from awareness using CFS[5]. This result would further suggest that some forms of figure-ground assignment might be possible in the absence of perceptual awareness, even while little visual input is able to reach higher areas of the ventral stream thought to mediate these processes.

Given the potential importance of this result, we first set out to replicate the result by Wang et al. (2012) using the b-CFS paradigm. We then tested two different sets of control stimuli to disentangle the contribution of the alignment of the edges in the image and the potential to perceive a surface induced by different variants of the Kanizsa configuration. Indeed, in the original Wang et al. study, no attempt was made to dissociate contributions of edge alignment from the perception of a surface. That is, for the configurations in which a surface could be perceived, the edges of the individual elements were always co-linear to each other (which is known to make a distinct contribution, see Conci et al., 2009). To control for the role of edge alignment in the image, we used a cross stimulus (instead of the standard pacmen) in which there is the same degree (or perhaps more) alignment between the edges in the image, but in which a Kanizsa surface is not normally elicited even when fully visible (Figure 4.1A, third column). If the suppression time difference observed in Wang et al. (2012) was due to the alignment of the edges rather than the construction of a surface (and engagement of figure-ground assignment), then the suppression time difference for a standard (aligned) and rotated (non-aligned) cross stimulus should be predicted to be similar to the one seen for the traditional Kanizsa stimulus. A second control stimulus was developed based on the stimuli used by Stanley and Rubin (2003), in which a surface percept

[5] Note that Wang et al. (2012) interpreted their results explicitly in terms of unconscious processing, the validity of which has been debated by Stein and Sterzer (2014) and by Gayet, van der Stigchel, and Paffen (2014).

is evoked by curved pacmen. The edges in these curved pacmen are obviously not collinear like those in the traditional Kanizsa or the cross stimuli, yet the stimulus still evokes a surface percept when fully visible (Figure 4.1A, middle column). If the potential for the stimulus to elicit a surface percept contributes to the suppression time difference observed for the traditional pacmen configuration, then the suppression time difference for the curved pacmen should be of a similar magnitude.

EXPERIMENT 1

In Experiment 1, we manipulated Stimulus Type (traditional pacmen, cross, or curved pacmen) and Configuration Type (standard or rotated). The comparison of a standard and rotated traditional pacmen configuration enables us to replicate the results from Wang et al. (2012), whilst the inclusion of a cross stimulus and curved pacmen (both also in standard and rotated configurations) enables us to test the relative contribution of edge collinearity and surface construction to any potential differences in suppression time. Furthermore, given that the surface percept in the curved pacmen configuration is weaker than the one elicited by the traditional pacmen configuration (Stanley & Rubin, 2003), one could predict that, if a surface is constructed during interocular suppression, breakthrough times would vary according to surface strength. Our predictions can be summarized as follows:

- If the configuration effect (standard vs. rotated) is due to the ability to induce a Kanizsa surface during CFS, we predict an effect of configuration that does not differ between the traditional versus curved pacmen, but does differ between traditional pacmen and cross stimuli, as well as between curved pacmen and cross stimuli.
- If the configuration effect (standard vs. rotated) is due to the existence of aligned edges, we predict an effect of configuration that does not differ between the traditional versus cross stimuli, but does differ between traditional and curved pacmen, as well as between cross stimuli and curved pacmen.

In all conditions observers were presented with a stimulus in one eye rendered invisible by the presentation of a flashing CFS stimulus (at 10 Hz) presented to the other eye. The critical dependent variable in all conditions was the time taken to 'break' this

suppression, such that participants could reliably report the location of the previously suppressed stimulus.

METHODS

Participants

20 people (age range: 18 – 30 years) participated in the experiment in exchange for monetary compensation. All participants had normal or corrected-to-normal vision and were naïve with respect to the goal of the study. The study was approved by the local ethical committee and all participants provided informed consent at the start of the experiment.

Apparatus

Stimuli were shown on two 19.8-in. Sony Trinitron GDM F500-R (2048 x 1536 pixels at 60 Hz, for each) monitors driven by a DELL Precision T3400 computer with an Intel Core Quad CPU Q9300 2.5 GHz processor running on Windows XP. Binocular presentation was achieved by a custom made stereo set-up. Two CRT monitors, which stood opposite to each other (distance of 220 cm), projected to the left and right eye respectively via two mirrors placed at a distance of 110 cm from the screen. A head- and chin rest (15 cm from the mirrors) was used to stabilize fixation. The effective viewing distance was 125 cm. Stimulus presentation, timing and keyboard responses were controlled with custom software programmed in Python using the PsychoPy library (Peirce, 2007, 2009).

Stimuli

The background of the display consisted of a random checkerboard pattern to achieve stable binocular fusion. The individual elements of the checkerboard were 0.34° by 0.34°. In both eyes, a white frame (10° by 10°) where the stimuli would be presented was superimposed on the checkerboard pattern. A black (eye dominance measurement) or red (main experiment) fixation cross was continuously present during the experiment (size 0.5° by 0.5°). In the eye dominance measurement phase, the target consisted of an arrow (maximal width 4°, maximal height 2°) and the CFS mask consisted of 150 squares with randomly picked sizes between 1° and 2° and a random luminance value.

The radius of the traditional pacman stimulus was 0.8° and the diagonal distance between the pacmen was 2°. The cross stimulus consisted of four crosses and was created such that the overall density of each cross matched that of the traditional pacmen stimuli (1.75° x 1.75°). The curved pacmen stimulus was created by superimposing two small discs (radius 0.34°) on the half of a full disc on which a white ellipse was drawn (major radius 1.19°, minor radius 0.37°). For all three stimuli, the area of the inner surface between the inducers was as equal as possible (exactly the same for the traditional pacmen and cross stimulus and slightly different for the curved pacmen stimulus). The rotated versions of the stimulus were generated by rotating the individual elements 45° with a jitter of +- 10° (see Figure 4.1A, middle row).

Given the importance in feature overlap between mask and stimuli in determining the effectiveness of CFS (Hong & Blake, 2009; Maehara et al., 2009; P. Moors et al., 2014; E. Yang & Blake, 2012), and the fact that the stimuli in the different conditions differed in various ways, we did not use a traditional Mondrian style CFS mask in the main experiment. Indeed, pilot results using a classical CFS mask indicated prolonged suppression for the cross condition, containing only vertical and horizontal orientations as in the traditional Mondrian CFS mask, indicating feature-selective depth of suppression (Yang & Blake, 2012). Therefore, we generated CFS masks that consisted of the individual elements of all different stimuli (see Figure 4.1B). This set of masks was generated before the start of the experiment (but was kept the same for every participant) and on each refresh (i.e., every 100 milliseconds) a new mask was selected from the pool with the restriction that the previous mask could not be used twice in a row.

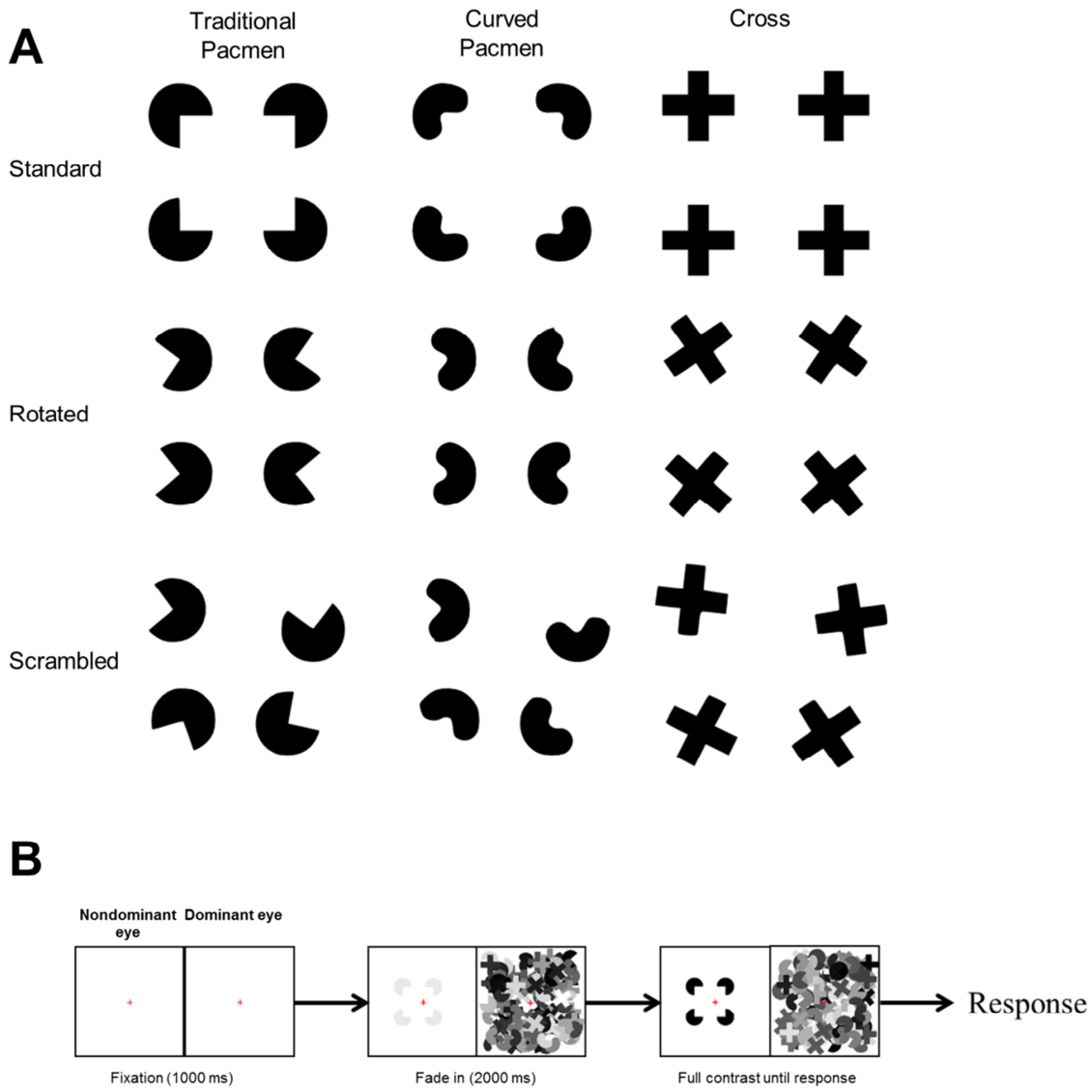

Figure 4.1. (A) Stimulus set used in Experiments 1 (two top rows) and 2 (all rows). (B) Basic trial sequence used in all experiments. A fixation cross was presented for 1000 ms after which the CFS mask was displayed in the dominant eye and the suppressed stimulus in the non-dominant eye. The suppressed stimulus was gradually increased in contrast over the course of 2000 ms after which it remained at full contrast until it broke suppression.

Procedure

Before starting the main experiment, participants' eye dominance was measured using the method set out by Yang, Blake, and McDonald (2010). On every trial, a fixation cross was presented for 1000 ms. Next, an arrow was presented in one eye and the CFS mask in the other (10 Hz refresh rate). Participants had to indicate the direction of an arrow as soon as it broke through the CFS mask by pressing "1" (for left) or "3" (for right) on a

numerical keyboard. The CFS mask was randomly presented to the left or right eye (40 trials per eye) and after 80 trials eye dominance was determined as the eye in which the mean suppression time of the arrow was lowest. In all subsequent parts of the experiment, the CFS mask was presented to the participants' dominant eye.

During the main experiment, participants were presented with the different stimuli from Figure 4.1A (first two rows only) of which the position relative to fixation was manipulated on each trial (i.e., shifted 0.5° left-, right-, up- or downwards). During the rest of the experiment, the participants always had to indicate as fast as possible in which direction the target stimulus was shifted by pressing "2", "4", "6", or "8" on a numerical keyboard for down, left, right, and up, respectively. Every trial started with a 1 second fixation period after which the target stimulus was presented to the non-dominant eye and the CFS mask to the dominant eye (refreshing at 10 Hz). The contrast of the target stimulus increased linearly from 0 to 100% over 2000 ms and then remained at full contrast until the stimulus was detected. Participants first completed a practice block in which the stimuli were presented without suppression in order to acquaint them with the task. In a second practice block, the CFS mask was presented together with the target stimuli. If everything was clear after the two practice blocks, participants began the actual experiment.

Design

The procedure to assess eye dominance included 80 trials. The two practice blocks contained 24 trials each. The main experiment consisted of 192 trials in total. The design of the main experiment was a full factorial 3 x 2 within-subjects design with three levels of the Stimulus Type factor (traditional pacmen, cross, and curved pacmen) and two levels of the Configuration Type factor (two levels: standard, rotated). The order of all experimental conditions was always randomized. After each sequence of 64 trials, participants were instructed to take a break of at least one minute.

RESULTS

Correct suppression times (95% of the data) were analyzed after log-transforming them due to the positive skew in the suppression time distributions. Outliers were defined as three times the standard deviation of the mean suppression time and these were removed

from the data set (2% of the data). Most of the outlying data points were presumably due to extremely effective suppression. The mean suppression times for each condition are summarized in Figure 4.2.

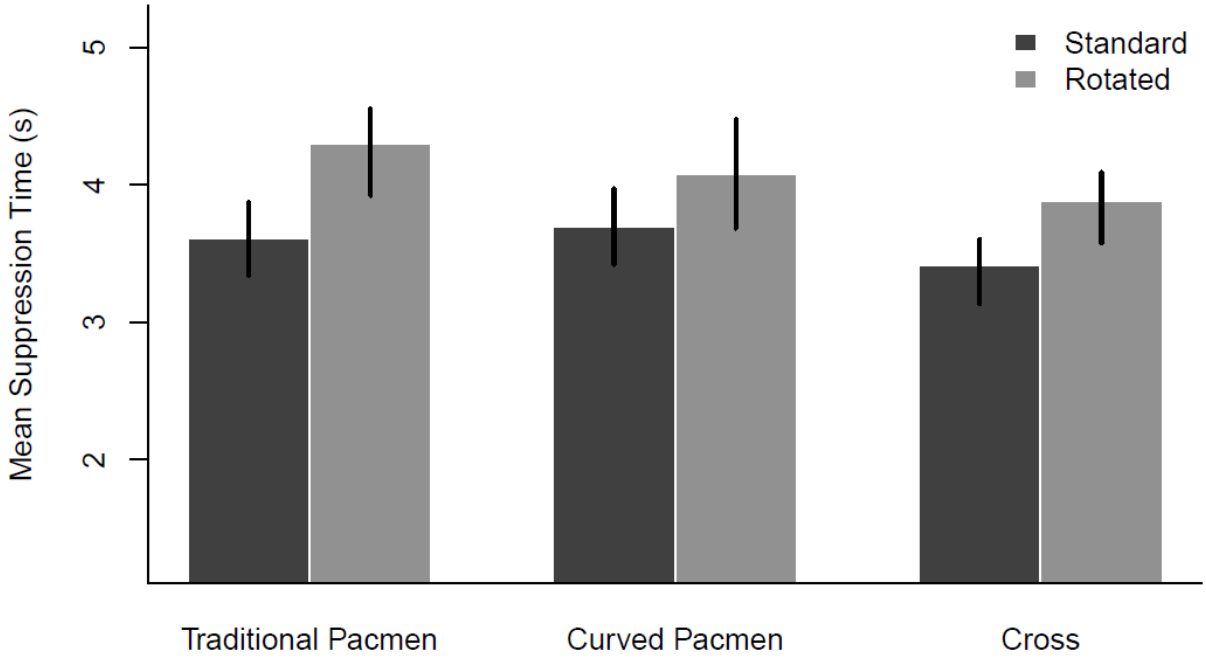

Figure 4.2. Results of Experiment 1. The bars depict the mean suppression time for each condition. The error bars denote 95% within-subject confidence intervals using the procedure set out by Morey (2008).

Statistical inference was done in a Bayesian framework relying on model selection using Bayes Factors (Rouder & Morey, 2012; Rouder, Morey, Speckman, & Province, 2012; Rouder, Speckman, Sun, Morey, & Iverson, 2009). The Bayes Factor quantifies the relative degree of evidence of one statistical model over another and therefore provides an intuitive measure to quantify the degree of belief in one statistical model over another (e.g., a model with and without a main effect of Configuration Type). The R package BayesFactor (version 0.9.9) was used to compute the Bayes Factors (Morey & Rouder, 2015). All considered models were ANOVA style models including random intercepts for participants, and the default settings for the priors (medium prior scale for fixed effects and nuisance prior scale for the participant effect). As a guideline to interpret the resulting Bayes Factors, we use the classification proposed by Jeffreys (1961) in that Bayes Factors from 3 onwards constitute substantial evidence for one model over the other.

The Bayes Factor analysis is summarized in Table 4.1. The best fitting model is depicted as having a Bayes Factor of 1 and all the other Bayes Factors can be interpreted as how much more likely the best model is compared to another model. As is apparent from Table 4.1, the preferred model includes main effects of both Stimulus Type and Configuration Type, and no interaction between these factors. Indeed, a model including an interaction is 53 times less likely as a model not including it. The main effect of Stimulus Type was largely due to the cross stimulus breaking suppression faster than the other stimuli. The main effect of Configuration Type indicates that standard stimuli broke suppression faster, on average, than rotated stimuli.

Table 4.1. Bayes Factor analysis for Experiment 1.

Model	**Bayes Factor**
ST + CT	1
ST + CT + ST*CT	53
All other models	> 100

Note. All Bayes Factors are relative to the best fitting model (i.e. in this case, the model of which the Bayes Factor is 1). All other models include all combinations from an empty to a full model not reported in the table. ST = Stimulus Type; CT = Configuration Type.

DISCUSSION

In Experiment 1, we replicated the suppression time difference between a standard and rotated version of the traditional pacmen stimulus, in line with the results of Wang et al. (2012). However, this configuration effect did not interact with the type of stimulus that was used. That is, it was statistically indistinguishable from the configuration effect observed in the curved pacmen and cross conditions. Therefore, the difference between the standard and rotated traditional pacmen conditions is presumably not driven by the potential for the stimulus to induce a surface percept or the alignment of the edges during interocular suppression. Indeed, irrespective of which process exactly contributes to the observed differences between conditions, the results of Experiment 1 indicate that the advantage for the standard configuration is *not specific* to the traditional pacmen configuration.

Given the pattern of results observed in Experiment 1, we were interested to further explore two potential explanations for the observed advantage for the standard configuration in a second experiment. That is, one could argue that the advantage for the standard Kanizsa was due to the configural relationship between the 4 elements. Alternatively, the observed advantage for the standard configuration could be due to independent contributions of the single elements comprising the configuration, based on their individual locations in the visual field and their orientation. For example, we know that observers are more sensitive to cardinal (horizontal and vertical) orientations compared to oblique ones (Appelle, 1972; Campbell & Kulikowski, 1966) and that these cardinal orientations are over-represented in the visual cortex (Li, Peterson, & Freeman, 2003; Yacoub, Harel, & Ugurbil, 2008). Thus, it could be that differences in the number of different orientations are directly influencing suppression strength.

Based on these two potential explanations, we included a scrambled version of each stimulus type in the stimulus set (Figure 4.1A, third row) and repeated the experiment in a different set of observers. This scrambled version was created by also manipulating the orientation of the individual elements as well as the distance from the center of the configuration. To more directly test whether differences were being driven by the differences between each of the individual elements (rather than their configuration) we also conducted a second experiment (with the same observers), in which we measured suppression times for the individual elements of each of the different configurations (cross, traditional pacman and curved pacman).

EXPERIMENT 2

The primary goal of Experiment 2 was to replicate the main finding from Experiment 1, that neither the ability to induce a Kanizsa surface, nor the collinearity of the lines increased the likelihood of breakthrough during CFS. The addition of two new conditions enabled us to ask two additional questions regarding the idea that surface completion mechanisms influence suppression times in CFS. First, a ‘scrambled’ condition was added to the ‘standard’ and ‘rotated’ conditions to test whether more deviation from the ‘standard’ configuration would further increase suppression times. Second, an ‘individual element’ condition was included for all of the stimulus configurations (‘standard’, ‘rotated’ and

'scrambled') to test whether any differences evident in the 'full configuration' might also be evident in the 'single element' condition. In addition to the predictions formulated for Experiment 1, our predictions for Experiment 2 can be summarized as follows:

- If the configuration effect observed in Experiment 1 reflects the configural relationship between the four elements, we predict that the scrambled configuration will yield slower breakthrough times compared to the standard and rotated configurations.
- If the configuration effect observed in Experiment 1 is due to the independent contributions of the four elements comprising the configuration, we predict that any effects of 'Configuration Type' will also be evident in the single element conditions, and that there will be no interaction between the Configuration Type and the Number of Elements (full configuration versus single elements).

METHODS

Participants

24 people (age range: 18 – 30 years) participated in the experiment and received monetary compensation for their participation. All participants had normal or corrected-to-normal vision and were naïve with respect to the goal of the study. The study was approved by the local ethical committee and all participants provided informed consent at the start of the experiment. 3 participants had to be excluded because suppression was too effective in either of the two sessions, with breakthrough times being three standard deviations longer than the mean value for all observers. One participant did not return for the second session and was therefore excluded from the analysis.

Apparatus

The experimental setup was the same as in Experiment 1.

Stimuli

The stimuli were the same as in Experiment 1, except that a scrambled version was added to the Configuration Type factor and a single element condition was developed. To create the scrambled condition, we picked four different angles relative to fixation at which

the individual elements would be drawn and four different orientations of the individual elements, kept constant across different Stimulus Types. We refer to this additional manipulation as the 'scrambled' configuration (in contrast to the 'standard' and 'rotated' conditions). This stimulus set is also depicted in Figure 4.1 (third row). It should be noted that only one 'scrambled' exemplar was created for this condition, being the one shown in Figure 4.1A. This choice allows us to avoid a difference in familiarity between the different stimuli because there is also only one exemplar in the other conditions.

In the single element condition we presented individual elements (just one pacman or cross rather than all four) at one of the same locations at which they were presented in the full configuration experiment.

Procedure

The experimental procedure was exactly the same as in Experiment 1, except for the session in which the individual elements of the stimuli were presented, in which the task was changed. In this condition, participants had to indicate in which quadrant of the display the stimulus was presented by pressing "1", "3", "4", or "6" for bottom left, bottom right, up left, and up right, respectively.

Design

The design of the experiment was a 3 x 3 x 2 full-factorial within-subjects design with three levels of the Stimulus Type factor (traditional pacmen, curved pacmen, and cross), three levels of the Configuration Type factor (standard, rotated, and scrambled) and two levels of the Number of Elements factor (full configuration and single element). Participants completed 288 trials in total for the main experiment in both the full configuration as well as the single element session. The practice blocks consisted of 36 trials each. After completing the first session, participants returned a week later to complete the second session. The ordering in which the sessions were completed was randomized across participants.

RESULTS

The data were analyzed in the same way as in Experiment 1. We removed outliers (full configuration session: 1.6%; single element session: 1.5%) defined as higher than three

standard deviations from the mean and analyzed only correct (full configuration session: 94%; single element session: 97%) suppression times after logarithmically transforming them. The results for the full configuration and single element sessions are depicted in Figure 4.3.

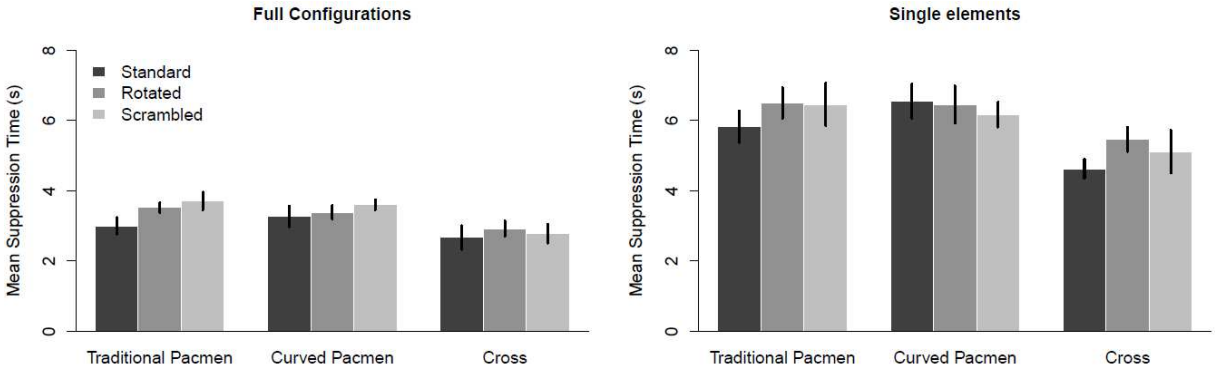

Figure 4.3. Results of Experiment 2. The bars depict the mean suppression time for each condition. The error bars denote 95% within-subject confidence intervals according to Morey (2008). (left) Full configuration session. (right) Single elements sessions.

A first goal was to assess whether the results were similar to those in Experiment 1. Therefore, we first report an analysis of only the conditions used in Experiment 1 (summarized in Table 4.2). As the analysis indicates, the preferred model is now one including an interaction between Stimulus Type and Configuration Type on top of the main effects of Stimulus Type and Configuration Type. Follow-up analyses indicated that this interaction was mostly driven by a tendency towards the absence of a configuration effect in the curved pacmen condition (BF = 2.5 in favor of the null model). Indeed, a configuration effect was still obtained in both the traditional pacmen as well as the cross condition (both BFs > 100 in favor of a configuration effect). Thus, if only a subset of the data similar to the data observed in Experiment 1 is considered, the data obtained in Experiment 2 indicate that the configuration effect is mostly driven by the existence of aligned edges in the traditional pacmen and cross configurations. We now turn to an analysis of the full data set.

Table 4.2. Bayes Factor analysis for the full configuration session of Experiment 2 (only the conditions used in Experiment 1 are included).

Model	Bayes Factor
ST + CT + ST*CT	1
ST + CT	19
All other models	> 100

Note. All Bayes Factors are relative to the best fitting model (i.e. in this case, the model of which the Bayes Factor is 1). All other models include all combinations from an empty to a full model not reported in the table. ST = Stimulus Type; CT = Configuration Type.

The next analysis focuses on the two predictions outlined for Experiment 2 (i.e., influence of scrambled configuration, and the difference between single and full element sessions). To assess the similarity of the results obtained in both single and full element sessions, we combined the data from both sessions and included an extra factor called Number of Elements in the analysis (i.e., full configurations vs. single elements). The results of the BF analysis are depicted in Table 4.3. As in Experiment 1, the best fitting model included main effects of Stimulus and Configuration Type, but now also included an interaction between those factors as well as a main effect of Number of Elements (i.e. stimuli consisting of four elements broke suppression faster than those consisting of a single element). However, it should be noted that the best model in the combined analysis was only slightly preferred (BF = 1.5) over a model including an interaction between Configuration Type and Number of Elements.

Table 4.3. Bayes Factor analysis for both experiment types combined.

Model	Bayes Factor
ST + CT + ST*CT + NE	1
ST + CT + ST*CT + NE + CT*NE	1.5
ST + CT + ST*CT + NE + ST*NE	20
ST + CT + ST*CT + NE + CT*NE + ST*NE	31
All other models	> 100

Note. All Bayes Factors are relative to the best fitting model (i.e. in this case, the model of which the Bayes Factor is 1). All other models include all combinations from an empty to a full model not reported in the table. ST = Stimulus Type; CT = Configuration Type; NE = Number of Elements.

Given that the BF analysis did not distinguish between the model with and without the interaction between Configuration and Number of Elements (BF = 1.5), we analyzed the more complex model further (i.e., the one including the interaction). Because the interpretation of this model was complicated by the interaction between Configuration and Stimulus Type, we probed the interaction between Configuration Type and Number of Elements for each Stimulus Type separately. This analysis explicitly addresses the question whether the number of elements (full configuration vs. single element) modulated the effect of Configuration for each Stimulus Type separately and is summarized in Table 4.4.

Table 4.4. Bayes Factor analysis of Configuration and Number of Elements, separately for each Stimulus Type.

Traditional pacmen		Curved pacmen		Cross	
Model	**Bayes Factor**	**Model**	**Bayes Factor**	**Model**	**Bayes Factor**
CT+ NE	1	**NE**	1	**CT + NE**	1
CT * NE	1.82	**CT * NE**	30	**CT * NE**	31
All other models	> 100	**All other models**	> 100	**All other models**	> 100

Note. All Bayes Factors are relative to the best fitting model (i.e. in this case, the model of which the Bayes Factor is 1). All other models include all combinations from an empty to a full model not reported in the table. CT = Configuration Type; NE = Number of Elements.

In the case of the traditional pacmen, the BF analysis does not distinguish between main effects of Configuration Type and Number of Elements or a model also including their interaction (BF = 1.82). When this interaction is further resolved (i.e., split up by Number of Elements), strong evidence for a configuration effect is found for the full configuration session (BF > 100), yet the reverse is observed in the single element session. Here, the BF indicates convincing evidence in favor of the null model (BF = 7). As is apparent from Figure 4.3, for the full configuration experiment, the mean suppression durations are in the direction predicted if the global configuration plays a key role (i.e., the scrambled configuration yielded the longest suppression durations). Although a similar pattern is observed in the single element experiment, the BF analysis indicated evidence for the absence of any differences between conditions.

In the case of the curved pacmen, the preferred model is one that includes a main effect of Number of Elements only. Here, the pattern of results is in the direction based on a prediction of the influence of the global configuration (in that the scrambled condition is slowest to break suppression) but interestingly this pattern is completely reversed in the single element session. Analyzing both experiments together, however, does not yield

sufficient evidence for an interaction between Configuration Type and Number of Elements and actually indicates no effect of configuration. Thus, for the curved pacmen, the results are in accordance with the prediction of the single-element account, in that the type of experiment did not interact with a potential configuration effect.

For the cross stimulus, the BF analysis indicates strong evidence for a model including a main effect of Configuration Type and Number of Elements, yet no interaction between both factors (BF = 31). In this case, the data are not consistent with the prediction that the scrambled condition would yield the slowest suppression durations (i.e., the rotated stimuli yielded the slowest durations). However, the absence of an interaction indicates that the configuration effect did not vary according to the Number of Elements, suggesting that the differences seen in the full configuration might be driven by the same processes influencing suppression in the single element condition.

DISCUSSION

The goal of Experiment 2 was to explore whether the configuration effect observed in Experiment 1 was due to the global characteristics of the stimulus or whether the differences could arise based on the differences in the individual elements that make up these configurations. To this end, a scrambled stimulus was included in Experiment 2 and participants were then tested in two experimental sessions, one in which the full configurations were presented and another in which the single elements of the configurations were used in isolation. We predicted that, if the global configuration was mostly responsible for the configuration effect observed in Experiment 1, the configurations of the scrambled condition would show slower suppression times compared to the standard and rotated stimulus configurations. Alternatively, if the single elements comprising each configuration were driving the configuration effect observed in Experiment 1, we predicted that the Number of Elements (full configuration vs. single element experiment) would not yield differential effects (interactions) with the Configuration Type.

The pattern of results that was observed in Experiment 2 could not be simply interpreted in either direction. That is, the stimuli that were generated for the scrambled condition yielded longer suppression times for the traditional pacmen only. In the case of the cross stimulus, the scrambled configuration yielded shorter suppression times compared to

the rotated configuration. For the cross, this pattern was also observed in the single element session. However, for the other two conditions (traditional and curved pacmen), the single element session was less readily comparable with the full configuration session. For the curved pacmen, the BF analysis indicated strong evidence for a main effect of Number of Elements only, indicating no differential pattern of results across both sessions (yet also no effect of Configuration Type). For the traditional pacmen, , the BF analysis indicated a potential interaction between Configuration Type and Number of Elements, although the results obtained in both sessions were qualitatively similar. When this interaction was further resolved, an effect of configuration was obtained only in the full configuration session. Furthermore, when the data were analyzed for the subset of stimuli used in Experiment 1 only, an interaction between configuration and Stimulus Type was observed. This interaction was primarily driven by the absence of a configuration effect for the curved pacmen. Thus, compared to Experiment 1, this subset of the data now indicated that aligned edges in the stimulus were primarily driving the configuration effect.

What is the most parsimonious interpretation of these results given the hypotheses laid out at the start of Experiment 2 and the results that were observed? We have clearly replicated one aspect of our findings from Experiment 1, namely that the ability for the stimuli to evoke a Kanizsa surface percept plays no role in breakthrough times. The fact that there was no consistent influence of scrambling for the three Stimulus Types also makes it hard to justify an effect based on the global configuration. On the one hand, there were some differences between the single element and full-configuration conditions, so the results in the full configurations cannot be fully predicted by the effects for the single elements. On the other hand, there was no interaction between Configuration Type and Number of Elements for the curved pacmen and the cross conditions, while qualitatively similar results were obtained for the full and single-element stimuli in the traditional pacmen condition. All in all, an account based on the features present in the individual elements and across the configuration as a whole seems to offer the most likely explanation for the effects of configuration observed here and in Experiment 1. Reflecting retrospectively on our results, it is worth pointing out that the visual system is most sensitive to cardinal orientations (horizontal and vertical), and less so to oblique ones (Appelle, 1972; Campbell & Kulikowski, 1966; Li et al., 2003; Yacoub et al., 2008). From this perspective, the configuration effects

observed in both experiments could potentially be explained by the differences between the orientations most prominently present in the stimuli, at the spatial scales of the individual elements and of the configuration as a whole. Indeed, for all Stimulus Types, the standard stimuli show strong peaks at the cardinal orientations in the Fourier orientation spectrum, compared to the rotated and scrambled stimuli (Figure 4.4). This difference at the cardinal orientations is most evident in the cross and traditional pacmen conditions, which can explain the consistent differences observed between the standard and rotated configurations. The configurations for the curved pacmen, on the other hand, show considerable overlap, which could explain why the results obtained for the curved pacmen condition are less consistent between experiments.

The stimuli that were generated for the scrambled condition include orientations in-between cardinal and oblique orientations, which is most pronounced in the orientation spectrum of the cross stimulus. In this respect, it makes sense that the scrambled cross configuration broke suppression faster compared to the rotated one (and similarly so for the single element condition). Furthermore, in the case of the single element condition, the orientation spectra of both the traditional and curved pacmen show strong overlap for all different configurations, which might explain the absence of a configuration effect for these stimuli in the single element condition. Moreover, one has to take into account that the visual inputs vary considerably between the full configuration and single elements, making it more difficult to obtain significant effects from potentially subtle differences between stimuli for the latter condition (unless they are strongly represented as in the cross configurations).

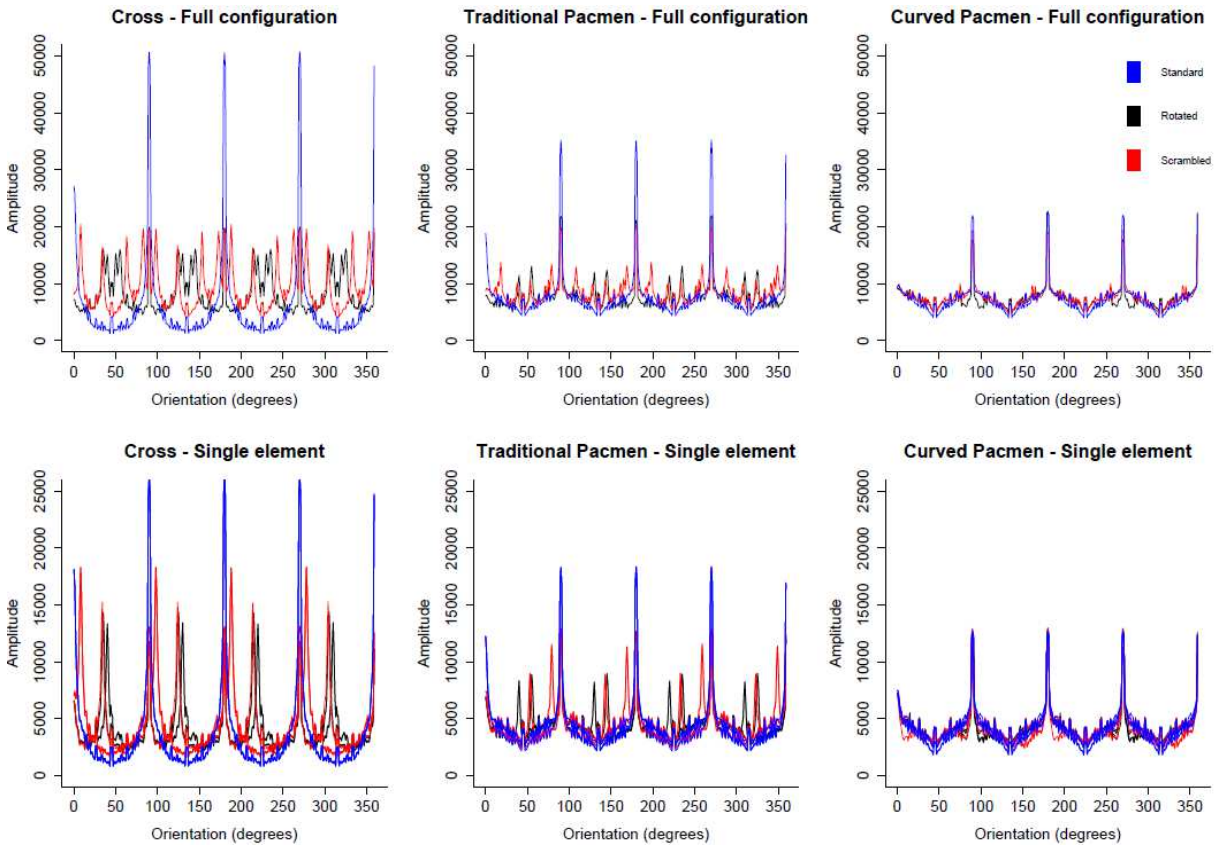

Figure 4.4. Fourier orientation spectrum for all stimuli. A fast Fourier transform was applied to all stimuli and the orientation spectrum was obtained by averaging the amplitude spectrum across all spatial frequencies for each orientation, separately.

GENERAL DISCUSSION

In this study, we set out to test whether the advantage of a traditional pacmen stimulus in breaking CFS (compared to a rotated control configuration) was due to the ability of that configuration to induce the perception of a surface or the existence of collinear edges in the image. Experiment 1 was a conceptual replication and extension of Wang et al. (2012). In addition to a traditional pacmen stimulus, two types of control stimuli were included. A cross configuration was used to assess whether the suppression time benefit of traditional pacmen configurations could be caused by the alignment of the edges rather than the ability for the stimulus to elicit a surface percept. Additionally, we included a configuration that consisted of curved pacmen. This stimulus elicited a surface percept but, critically, none of the contours of the inducers were collinear with each other. The use of these two types of control stimuli enabled us to test whether the suppression time difference observed in Wang et al. (2012) could be explained by a genuine surface-based figure-ground

assignment process (evident in the curved pacmen stimulus) or rather by the local alignment of edges in the inducers (evident in the cross stimulus). The results of Experiment 1 showed a clear replication of the suppression time difference between standard and rotated traditional pacmen stimuli observed in Wang et al. (2012), but, this effect did not distinguish between the stimulus types employed. That is, in all conditions a difference between a standard and rotated global configuration was observed. This observation indicates that the suppression time difference in the traditional pacmen condition was not specific to the potential for the stimulus to induce a surface percept.

As a follow up, we conducted a second experiment in which we explored whether the global configuration was driving the observed differences between conditions in Experiment 1 or, alternatively, that the differences were being driven by the differences between the single elements belonging to these configurations. To this end, we used the same stimuli as in Experiment 1 with the addition of a set of scrambled stimuli (by manipulating the distance and orientation of the individual elements relative to the fixation cross, as well as their individual orientation) and tested participants in two sessions. In one of them, the full configurations were used as stimuli. In the other, on each trial a single element of one of the configurations was presented. If the configuration effect observed in Experiment 1 was due to the global stimulus configuration, we predicted that the scrambled stimulus configuration would elicit slower suppression times compared to the rotated configuration. Alternatively, if the elements of the configurations were driving the differences, we predicted to observe a similar pattern of results in both full and single-element experimental sessions.

The results revealed no consistent longer suppression durations for the scrambled configurations compared to the standard and rotated ones. Furthermore, although the data were qualitatively similar between the full configuration and single element sessions (except for the curved pacmen), no conclusive evidence was obtained that the results of the single element session were the same as in the full configuration session. Based on these results, we argued that a possible account for the data could be the differences in low-level stimulus features such as orientation. That is, cardinal orientations – to which the visual system is most sensitive – were more strongly represented in the stimuli that broke suppression fastest and these differences were most pronounced in the full configuration experiment because of the presentation of the simultaneous presentation of four rather than one element.

Our study was motivated by the observation that there was a discrepancy between the findings of Sobel and Blake (2003) and Harris et al. (2011) on the one hand and Wang et al. (2012) on the other. Although we replicated the findings reported in the latter study, the overall pattern of our results clearly supports the conclusions advanced in the former studies. That is, our results fail to provide evidence that, during CFS, traditional pacmen stimuli can induce figure-ground processes that might lead to a differential effect for stimuli able to induce a surface percept. This observation is consistent with a broader set of recent studies focusing on the extent to which mid- and high-level stimuli are represented during CFS (Faivre & Koch, 2014a; Gayet et al., 2014; Hedger, Adams, & Garner, 2015a; Hesselmann & Knops, 2014; Hesselmann & Moors, 2015; Heyman & Moors, 2014; P. Moors, Huygelier, Wagemans, de-Wit, & van Ee, 2015). That is, there is converging evidence that suppressed stimuli are processed to a limited extent during CFS and that any process that requires complex integration of several features of the suppressed stimulus is unlikely to take place. Given that the construction of a Kanizsa surface percept not only requires grouping, but also figure-ground assignment, it is logical to predict that it should not manifest under CFS. This is furthermore reinforced by the studies reporting on neural activity associated with stimuli suppressed during CFS (Fogelson, Kohler, Miller, Granger, & Tse, 2014; Hesselmann & Malach, 2011; Ludwig, Kathmann, Sterzer, & Hesselmann, 2014; Sterzer, Stein, Ludwig, Rothkirch, & Hesselmann, 2014; Yuval-Greenberg & Heeger, 2013). Invariably, these studies show that activity related to the suppressed stimulus is limited to early visual areas such as V1 and V2. Parallel work on the Kanizsa stimulus, from functional neuroimaging studies (Seghier & Vuilleumier, 2006; Stanley & Rubin, 2003), neuropsychological research (de-Wit et al., 2009), and comparative studies with monkeys (Huxlin, Saunders, Marchionini, Pham, & Merigan, 2000) suggests that higher areas in the ventral stream (and the lateral occipital complex in particular) are critically involved in the construction of the surface percept for traditional pacmen stimuli. Thus, combining this knowledge on the extent to which CFS should block information transfer to higher areas, and the role of higher ventral areas in the construction of the Kanizsa surface percept the results reported in this study are not surprising. Furthermore, it highlights the importance of using a range of control conditions to be sure that any difference observed between experimental conditions is actually the result

of the perceptual difference elicited by the stimuli used in those conditions, especially for perceptually compelling phenomena like the Kanizsa surface.

Given that the relationship between perceptual organization and awareness necessarily involves the choice of a suitable paradigm to render a stimulus invisible, it remains plausible that Kanizsa surfaces *can* be constructed in the absence of visual awareness depending on the nature of suppression of the paradigm that was employed (Breitmeyer, 2015; Breitmeyer, Koç, Oğmen, & Ziegler, 2008; Hesselmann & Moors, 2015). Indeed, using visual masking, a paradigm for which it has been argued that the initial feedforward transfer of input to higher areas, there is evidence that masked Kanizsa like stimuli can influence performance on a subsequent shape discrimination task (Poscoliero et al., 2013). Similarly, a recent neuroimaging study relying on an inattentional blindness paradigm reported evidence for similar processing of Kanizsa figures for groups of participants that either could or could not distinguish a Kanizsa figure in a forced-choice task after having performed an attentionally demanding task (and, hence, had been inattentionally blind to the Kanizsa figure). Moreover, neuropsychological studies relying on patients suffering from visuospatial neglect, a condition mainly caused by stroke in the parietal region, also provided evidence for processing of illusory shapes and contours (Conci et al., 2009; Driver & Mattingley, 1998; Mattingley, Davis, & Driver, 1997).

Thus, the main message of this study is not necessarily that one needs to be aware of a visual stimulus for some form of perceptual grouping and figure-ground assignment to take place, but that one needs to consider the specific mechanisms by which a particular paradigm renders stimuli invisible. Depending on the level at which suppression takes place, one might reasonably hypothesize that a process can happen in the absence of visual awareness. For CFS or other binocular suppression techniques, however, it seems highly likely that the processing of a suppressed stimulus is rather limited.

ACKNOWLEDGMENTS

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METH/14/02). RVE is supported by FWO and the EU Horizon 2020 program (HealthPac). LDW is supported by FWO through a postdoctoral fellowship.

FILE DRAWER

Important disclaimer

This material has NOT been peer reviewed. The level of detail on the experiments is NOT comparable to what should be reported in a paper. I intend to fix this in the future, but due to time constraints, I'm currently writing down everything in a general form.

Introduction

Every published study highly likely has its fair share of unreported (sometimes failed) experiments. I have always tried to include every experiment (which has the status of being more than a pilot experiment) in my published papers. For various reasons (outlined below), this has not happened for the breaking CFS study on the Kanizsa stimulus, recently published in Attention, Perception, and Psychophysics (Moors, Wagemans, van Ee, & de-Wit, 2016). I have thought a lot on whether to report on these unpublished experiments. I do think however they have some value for further interpretation of the results reported in our published paper. Thus, in this document, I will report on two additional experiments we have performed in the context of our recent study. I will sketch the context in which these experiments were performed, why they were not included in the paper, and will report on their results, and briefly discuss their implications.

Experiment 1

This results of this first experiment actually inspired us to perform the first experiment reported in our AP&P paper, and they have been presented at the European Conference on Visual Perception 2013.

The context in which this experiment was set up was that we were considering whether CFS could be used as some kind of "simulation" of visuospatial neglect, by presenting the CFS mask to the left half of the visual field. We were inspired by the study of Conci et al. (2009), in which it was shown that both contours and surfaces contributed to the reduction in extinction for regular versus random Kanizsa stimuli that was previously observed in

participants with hemispatial neglect. We reasoned that, for CFS to be a viable model for neglect, it should be possible to replicate these results in a CFS experiment. Thus, we set up an experiment in which we used a half field CFS mask to suppress the left half the Kanizsa stimuli that were similar to the ones in the study of Conci et al., which were the following four stimuli:

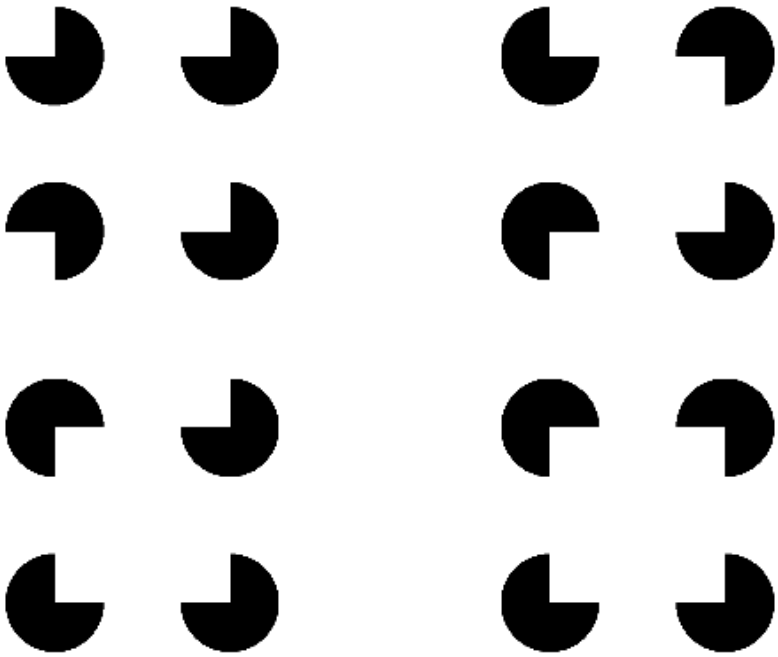

Figure S4.1. Stimulus set used in Experiment 1. (top left) Random stimulus, (top right) Contour stimulus, (bottom left) Partial surface stimulus, (bottom right) Regular stimulus.

The upper left stimulus is referred to as the "random" stimulus, the upper right one is the "contour" stimulus, the lower left one the "partial surface" stimulus, and the lower right one the "regular" stimulus. The idea behind the stimuli is that the full-blown surface percept is incrementally built up going from random to Kanizsa (with collinearities between the pacmen in the contour stimulus, and a partial surface in the, well, partial surface stimulus). We designed the stimuli such that the visible part (the right part) never could fully predict what would be presented in the suppressed half of the stimulus. We presented these stimuli in a standard b-CFS experiment, in which the contrast was gradually increased and participants had to indicate when the left part of the stimulus broke suppression. To ensure participants were not randomly responding, they had to perform a task on the pacmen that

entered awareness. That is, upon breakthrough, participants had to indicate in which direction both pacmen were pointing. The order (upper or lower first) was counterbalanced across participants. Because this wasn't an easy task, participants were allowed to practice it extensively on the fully visible stimuli. The trial structure in this experiment was as follows:

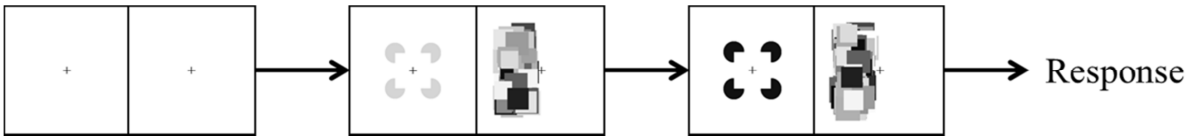

Figure S4.2. Basic trial sequence used in the experiment. A fixation cross was presented for 1000 ms after which the half-CFS mask was displayed in the dominant eye and the suppressed stimulus in the non-dominant eye. The suppressed stimulus was gradually increased in contrast over the course of 2000 ms after which it remained at full contrast until it broke suppression. As soon as the pacmen became visible, participants had to indicate the pointing direction of the pacmen.

We tested 19 participants in total, three of which were removed due to poor performance on the task. The experiment consisted of 300 trials in total (75 trials in each condition). Trials were presented in a completely random order, and participants were allowed to take breaks after each quarter of the trials was completed. Mean suppression times suppression times for all conditions are depicted in the following figure:

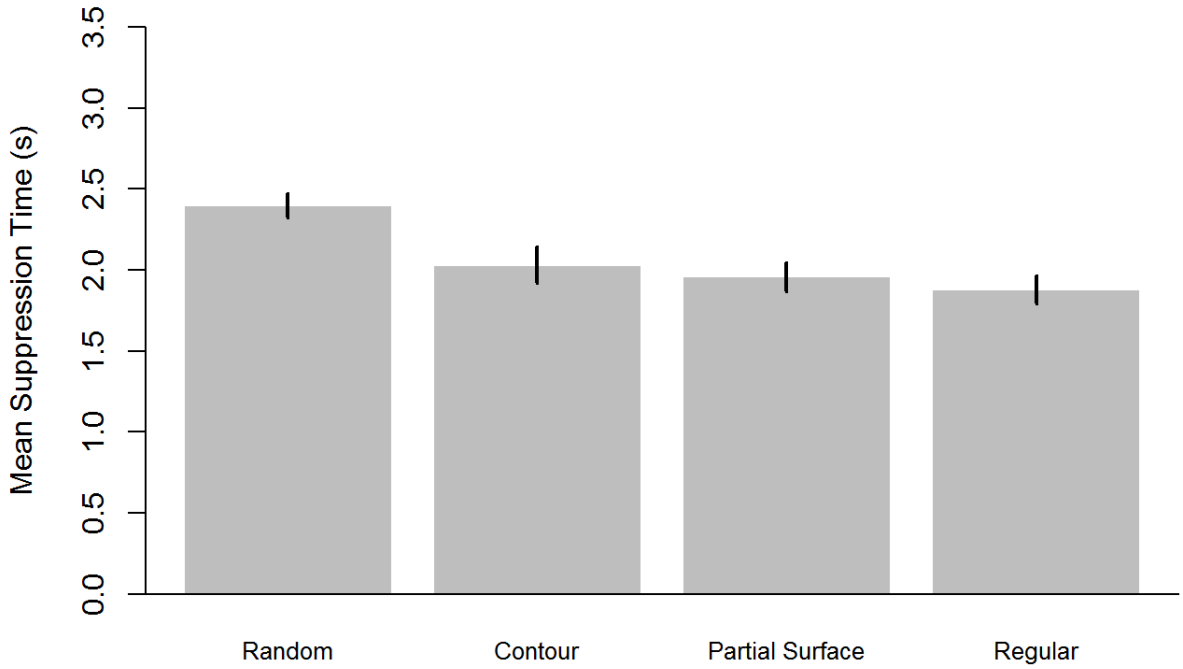

Figure S4.3. . Results of Experiment 1. The bars depict the mean suppression time for each condition. The error bars denote 95% within-subject confidence intervals using the procedure set out by Morey (2008).

Statistical analyses indicated that the regular condition was not different from the partial surface condition, yet was different from the contour and random condition. The partial surface condition then did not differ from the contour condition, but only from the random condition and similary for the contour condition. Although we interpreted these results as replicating those of Conci et al. (2009), we also realised that a simple explanation based on the collinearities in the stimulus (none for the random, two crossing from the right to the left hemifield in the contour condition, one in the suppressed part of the stimulus in the partial surface and the contour condition). So, according to us, it seemed to be the case that "long-range" collinearities in the stimulus could equally well explain the results, and that we did not have to invoke an explantion based on a contribution of surfaces. Therefore, we designed the experiment which is reported as a first experiment in our AP&P paper.

Why didn't we report on this experiment? Well, first of all, we used a half field CFS mask of which reviewers might ask why we used it in the first place. Given that the focus of our paper was not on the relationship between suppression through CFS and suppression in visuospatial neglect, we thought this would be difficult to motivate. Secondly, the task we used allowed for some extensive processing when the stimulus entered awareness. Indeed,

participants had to perform a task on the pacmen, rather than on their location (which we fixed in our reported experiments). The combination of these two factors made us reason it would be difficult to meaningfully integrate this experiment into a coherent line of reasoning. But hey, here it is, for all of us to see and comment on.

Experiment 2

The second unreported experiment is quite similar to the "full configurations" experiment we reported as part of Experiment 2. In this experiment, we did not yet include the "single elements" part of the experiment, but wanted to test the effect of further randomizing the Kanizsa stimuli. Due to a programming error, however, I only manipulated the rotation of the pacmen, but not the distance to the fixation cross. Therefore, we had to run this experiment again, and in the meantime we also included the part in which we only showed the single elements. Nevertheless, this experiment was not a failed experiment, and the results are equally interesting to report. So essentially, the experiment is the same as the full configurations one reported in Experiment 2, but I failed to properly manipulate the stimuli as described in the methods secition of Experiment 2. I did randomize the orientations of the pacmen, but I did not randomize the distance to fixation.

We tested 20 participants, who completed 288 trials in total (32 trials per condition). The data were analyzed in the same way as reported in our published paper. The mean suppression times looked as follows:

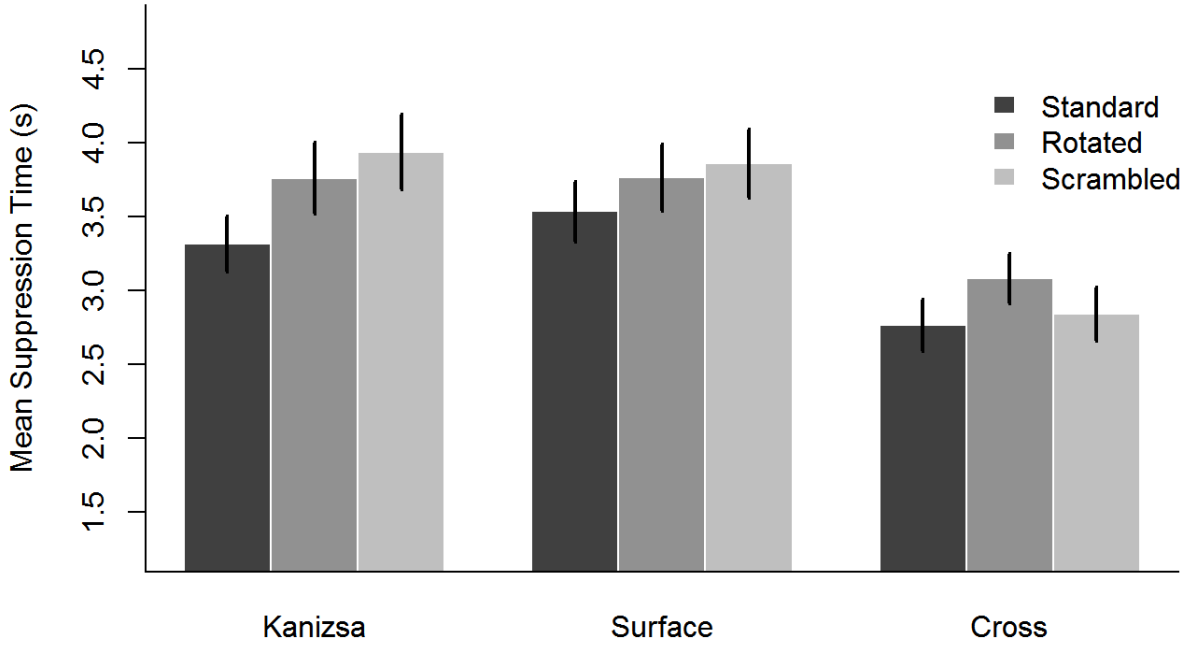

Figure S4.4. . Results of Experiment 2. The bars depict the mean suppression time for each condition. The error bars denote 95% within-subject confidence intervals using the procedure set out by Morey (2008).

The analysis on the conditions that were included in Experiment 1 (of the paper) revealed the same result (in contrast to what we observed in Experiment 2 of the paper). That is, the Bayes Factor for a model including main effects of stimulus type and configuration type compared to a model including the main effects and their interaction was 77.

The analysis including the scrambled condition revelead a similar result, yet the Bayes Factor now was reduced to 3, highlighting that there is only moderate evidence for the absence of an interaction effect. In sum, the results obtained in this unreported experiment replicate the results of Experiment 1, and are somewhat inbetween compared to the results we obtained in Experiment 2. That is, for the cross and traditional pacmen conditions, we obtained similar results. For the curved pacmen condition, the result was in the reverse direction (compared to Experiment 2).

Short Discussion

I think these results add to and provide further evidence for the main interpretation of the results of our published paper. That is, in both experiments, we do not find any

convincing evidence that the difference in suppression time between a standard and rotated traditional pacmen stimulus is due to presence of an (illusory) surface.

Chapter 5.
Suppressed visual looming stimuli are not integrated with auditory looming signals: evidence from continuous flash suppression

Previous studies using binocular rivalry have shown that signals in a modality other than the visual can bias dominance durations depending on their congruency with the rivaling stimuli. More recently, studies using continuous flash suppression (CFS) have reported that multisensory integration influences how long visual stimuli remain suppressed. In this study, using CFS, we examined whether the contrast thresholds for detecting visual looming stimuli are influenced by a congruent auditory stimulus. In Experiment 1, we show that a looming visual stimulus can result in lower detection thresholds compared to a static concentric grating, but that auditory tone pips congruent with the looming stimulus did not lower suppression thresholds any further. In Experiments 2, 3 and 4, we again observed no advantage for congruent multisensory stimuli. These results add to our understanding of the conditions under which multisensory integration is possible, and suggest that certain forms of multisensory integration are not evident when the visual stimulus is suppressed from awareness using CFS.

Moors, P., Huygelier, H., Wagemans, J., de-Wit, L., van Ee, R. (2015). Suppressed visual looming stimuli are not integrated with auditory signals: evidence from continuous flash suppression. *i-Perception*, 6(1), 48-62.

INTRODUCTION

When both eyes are presented with incompatible images, the phenomenological consequence usually is one of binocular rivalry in which the percept alternates between the two images instead of mixing them into one coherent interpretation (Blake & Logothetis, 2002; Tong et al., 2006). Alternations between rivaling percepts can be modulated by numerous factors, including the contrast (Fox & Rasche, 1969) or spatial frequency (Fahle, 1982) of the stimuli, the allocation of attention (Meng & Tong, 2004; Ooi & He, 1999; Paffen, Alais, & Verstraten, 2006; van Ee, van Dam, & Brouwer, 2005), and stimulus predictability (Chopin & Mamassian, 2012). In addition, it has become clear that non-visual stimuli can influence the rivalry between visual stimuli, from audition (Alais, van Boxtel, Parker, & van Ee, 2010; Chen, Yeh, & Spence, 2011; Conrad et al., 2013; Conrad, Bartels, Kleiner, & Noppeney, 2010; Guzman-Martinez, Ortega, Grabowecky, Mossbridge, & Suzuki, 2012; Kang & Blake, 2005; van Ee, van Boxtel, Parker, & Alais, 2009), to touch (Lunghi & Alais, 2013; Lunghi, Binda, & Morrone, 2010; Lunghi, Morrone, & Alais, 2014; Lunghi & Morrone, 2013), and even olfaction (Zhou, Zhang, Chen, Wang, & Chen, 2012; Zhou, Jiang, He, & Chen, 2010). Most of these studies have demonstrated that the influence of multisensory stimulation from the auditory modality specifically increases the duration of the already dominant (conscious) stimulus, rather than causing visual perception to switch to a non-dominant (unconscious) stimulus (Chen et al., 2011; Conrad et al., 2010; Kang & Blake, 2005). However, some evidence has also been reported for an increased probability of switching to the currently non-dominant stimulus when it was congruent with an auditory (Lunghi et al., 2014) or tactile signal (Lunghi & Alais, 2013; Lunghi et al., 2010, 2014; Lunghi & Morrone, 2013).

The question whether multisensory integration can be achieved for a stimulus suppressed from visual awareness through interocular suppression has recently been readdressed in studies in which continuous flash suppression (CFS) was used as the interocular suppression paradigm (Tsuchiya & Koch, 2005; Tsuchiya, Koch, Gilroy, & Blake, 2006). CFS is a binocular rivalry variant in which a dynamic noise pattern (usually consisting of shapes of random size and orientation, called a CFS mask) is presented to one eye. In most implementations, the mask content refreshes every 100 milliseconds (ms) (i.e., at 10 Hz), yielding robust suppression of the stimulus presented to the other eye. CFS provides

interesting advantages over the use of regular binocular rivalry for assessing multisensory integration in the absence of visual awareness. The CFS mask is usually dominant at stimulus onset, enabling stricter control over which stimulus dominates in visual awareness at the start of each trial. This provides the opportunity to assess specifically whether the suppressed stimulus is integrated with the stimulus presented in the non-visual modality.

Studies in which CFS was used as the interocular suppression paradigm have reported evidence that multisensory integration can occur in the absence of awareness (Alsius & Munhall, 2013; Palmer & Ramsey, 2012; Plass, Guzman-Martinez, Ortega, Grabowecky, & Suzuki, 2014; Salomon, Lim, Herbelin, Hesselmann, & Blanke, 2013; Yang & Yeh, 2014; Zhou et al., 2010). Most of these studies have relied on breaking CFS (b-CFS) (a term coined by Stein, Hebart, & Sterzer, 2011, based on a paradigm introduced by Jiang, Costello, & He, 2007) in which a stimulus is suppressed through CFS and gradually increased in contrast until it "breaks through" the CFS mask (i.e., becomes detectable). Upon breakthrough, participants usually have to perform a speeded localization task on the stimulus. Differential suppression times for different stimuli are then attributed to differences in stimulus processing during suppression (e.g., as in Jiang et al., 2007). For example, Zhou et al. (2010) reported that the congruency between an olfactory stimulus (i.e., the smell of a rose or a marker) and a visual stimulus (i.e., an image of a rose or a marker) suppressed by CFS can bias suppression times such that congruent stimuli break through suppression faster than incongruent stimuli. Similarly, Alsius and Munhall (2013) reported that the congruency relation between auditory stimuli and a visual lip-stream sequence suppressed from awareness by CFS modulates suppression times such that the congruent stimulus combination breaks through suppression faster. These findings seem to indicate that multisensory integration can indeed take place in the absence of awareness of one of the modalities (the visual one) and that the supraliminal modality can bias the breakthrough times of the suppressed stimulus. However, these studies have relied on b-CFS, and the validity of this paradigm to assess unconscious processing of the suppressed stimulus has recently been questioned (Stein, Hebart, et al., 2011; Stein & Sterzer, 2014). Because the responses in the b-CFS paradigm rely on the participants being conscious of the stimulus of interest, it is necessary to ensure that the difference in suppression times is driven by a difference in the time at which stimuli break through suppression. That is, different stimuli

could break through suppression on average at the same time, yet the critical stimulus manipulation could influence the participants' response time to one of the stimulus classes. This would yield a difference in suppression times that is not attributable to differences in processing during suppression, but rather to post-perceptual or decisional factors. To rule this out, a control condition is traditionally used in which the CFS mask and the stimulus are both presented to both eyes (binocular control condition in which no interocular suppression takes place). However, this control condition has been shown to be insufficiently comparable to the CFS condition to infer unconscious processing (Stein, Hebart, et al., 2011). This has led Stein and Sterzer (2014) to argue that b-CFS, as it is currently used, cannot provide evidence for unconscious processing.

The present study

In this study, we set out to reevaluate whether multisensory integration can be achieved between a suppressed visual stimulus and a supraliminal auditory stimulus using a different paradigm than b-CFS. To do so, we tested whether a visual looming stimulus (as previously used by van Ee et al., 2009), while being suppressed by CFS, can be integrated with a concurrently presented tone pip or looming sound, by measuring detection thresholds of the visual looming stimulus in different conditions. As in the previous studies on multisensory integration during interocular suppression, we opted to use CFS because it can ensure that the visual looming stimulus is suppressed at trial onset. To avoid the problems associated with b-CFS, we fixed the presentation time of the stimuli and measured contrast detection thresholds for the visual looming stimulus. Such an accuracy-based measure has been used in previous studies (Kaunitz, Fracasso, Lingnau, & Melcher, 2013; Stein et al., 2011; Tsuchiya et al., 2006; van der Groen, van der Burg, Lunghi, & Alais, 2013; Yang & Blake, 2012) and largely avoids the potential problem of differential response criteria in the classic implementation of b-CFS (Stein, Hebart, et al., 2011). Furthermore, we reasoned that if audiovisual integration indeed takes place in the absence of awareness of the visual looming stimulus, suppression strength in this condition would be lower than in the other conditions (Kang & Blake, 2005). Consequently, the thresholds in this condition would be expected to be lower compared to an incongruent or visual only condition.

We were interested in testing whether a suppressed visual looming stimulus could be integrated with a supraliminal auditory (looming) stimulus not only because looming is a biologically relevant signal that might be crucial for survival (Bach, Neuhoff, Perrig, & Seifritz, 2009; Fotowat & Gabbiani, 2011; Ghazanfar, Neuhoff, & Logothetis, 2002; Maier, Neuhoff, Logothetis, & Ghazanfar, 2004; Neuhoff, 1998, 2001; Schiff, Caviness, & Gibson, 1962), but also because previous studies already have demonstrated that a looming stimulus dominates perception in binocular rivalry and that attentional allocation to a rhythmically congruent auditory looming signal can boost the attentional effect of holding the looming stimulus in perceptual dominance (Conrad et al., 2013; Parker & Alais, 2007; van Ee et al., 2009). Furthermore, neuropsychological evidence suggests that looming stimuli can be processed in the absence of awareness. That is, extinction due to brain damage has been reported to be less severe for looming stimuli compared to contracting stimuli (Dent & Humphreys, 2011). Lastly, with respect to the neural locus of audiovisual looming integration, a recent fMRI study by Tyll et al. (2013) documented super-additive fMRI responses in visual cortex for multisensory looming signals compared to unisensory signals. Furthermore, the authors reported enhanced functional connectivity between the superior temporal sulcus and lower-level visual areas during multisensory looming stimulation. Since multisensory looming signals appear to be processed at least partially in early visual cortex, this finding prompts the question as to whether CFS completely abolishes the potential for audiovisual looming integration or whether the visual looming signal is sufficiently preserved to allow for the auditory signal to interact with the visual looming signal.

In sum, based on the available evidence on multisensory integration between a suppressed visual stimulus and a suprathreshold stimulus of the non-visual modality, the present study addressed the extent to which a suppressed visual looming stimulus could be integrated with an auditory (looming) signal. This question was particularly motivated by the biological relevance of looming, the available neuropsychological evidence on audiovisual looming integration in brain-damaged patients as well as the neural locus of fully visible audiovisual looming stimuli. Furthermore, by measuring contrast detection thresholds of the visual looming stimulus in different experimental conditions, we were able to explicitly test whether auditory stimulation directly influenced the strength of the suppressed stimulus. We conducted four experiments to assess whether an auditory

(looming) signal influenced contrast detection thresholds of a visual looming stimulus. To preview our results, we obtained no evidence in favor of audiovisual integration of suppressed visual looming stimuli and auditory tone pips (Experiments 1, 2, and 4) or looming sounds (Experiment 3).

EXPERIMENT 1

The goal of Experiment 1 was to assess whether visual looming per se would lead to lower contrast detection thresholds compared to a static concentric grating. Secondly, we sought to test whether an auditory tone pip could further lower contrast detection thresholds when presented in a rhythmically congruent fashion with the visual looming stimulus. Thus, Experiment 1 consisted of three conditions: visual static, visual looming, and audiovisual looming. On each trial, participants had to indicate whether the target stimulus, initially suppressed from visual awareness through CFS, was presented above or below fixation and contrast detection thresholds were measured using a QUEST staircase procedure (Watson & Pelli, 1983).

Methods

Participants. Seventeen participants took part in this study, two of which were the first authors. All participants had normal hearing and normal or corrected-to-normal vision. The mean age was 22.5 years (*SD* = 1.71). All participants signed an informed consent. All experiments were conducted in line with the ethical principles regarding research with human participants as specified in The Code of Ethics of the World Medical Association (Declaration of Helsinki). The study was approved by the Ethical Committee of the Faculty of Psychology and Educational Sciences (EC FPPW) of the University of Leuven.

Apparatus and stimuli. All experiments were conducted in a dark room. The stimuli were created with PsychoPy (Peirce, 2007, 2009) and presented on two gamma-corrected color cathode ray tube (CRT) monitors (2048 x 1536 resolution for each monitor). Participants viewed a pair of dichoptic displays through a mirror stereoscope at a distance of 125 centimeters with a chin rest. The refresh rate of the monitor was 60 Hz. A checkerboard pattern was continuously presented to ensure binocular fusion (checker size 0.34°). A fixation cross with (0.6° x 0.6°) was continuously presented in the center of the screen.

In the eye dominance measurement phase, the target stimulus was an arrow (maximal length 4°; maximal height 2°) pointing leftwards or rightwards which was presented at the center of the screen. A grayscale CFS mask consisting of Mondrian patterns was presented against a uniform grey background at mean luminance (25 cd/m^2) with a frame size of 5° x 5°. The CFS mask consisted of 150 elements (squares) presented within a range of 3° x 3° with a contrast (root mean square) of 27.5 % and refreshed every 0.10 seconds (i.e. at 10 Hz). The position and size of the mask elements were randomly alternated. The size of the elements ranged from 1° to 2°.

For the main part of the experiment, visual stimuli were presented on a uniform grey background with a size of 2.5° x 2.5° at mean luminance (see Figure 5.1 for an example of the trial sequence). The target stimulus consisted of a concentric sine wave grating with a size of 1° and a spatial frequency of 3 cycles per degree. The stimulus was either presented static or with looming motion. The appearance of looming was created by phase shifts. The magnitude of the phase shift was increased exponentially over a 0.8 second period from 0 to 4 Hz and was slowed down according to a cosine decay for 0.2 seconds such that one looming cycle lasted 1 second (i.e., looming motion at 1 Hz). This implementation of looming motion was based on stimuli used in previous experiments (Parker & Alais, 2007; van Ee et al., 2009). The auditory stimulus was a pure tone of 200Hz that was presented together with the visual looming stimulus at the peak of each looming cycle for 0.28 seconds (the amplitude of the tone was modulated such that it peaked after 0.14 seconds). The CFS mask consisted of 50 circular mask elements alternating randomly in position and size (within a range of 1° and 2°). The mask was refreshed every 0.10 seconds. The CFS mask was presented in the left half of the visual field (1.5° x 1°; 27.5 % contrast, root mean square). The target stimulus was always presented 1.25° left of fixation and either 1.25° above or below fixation.

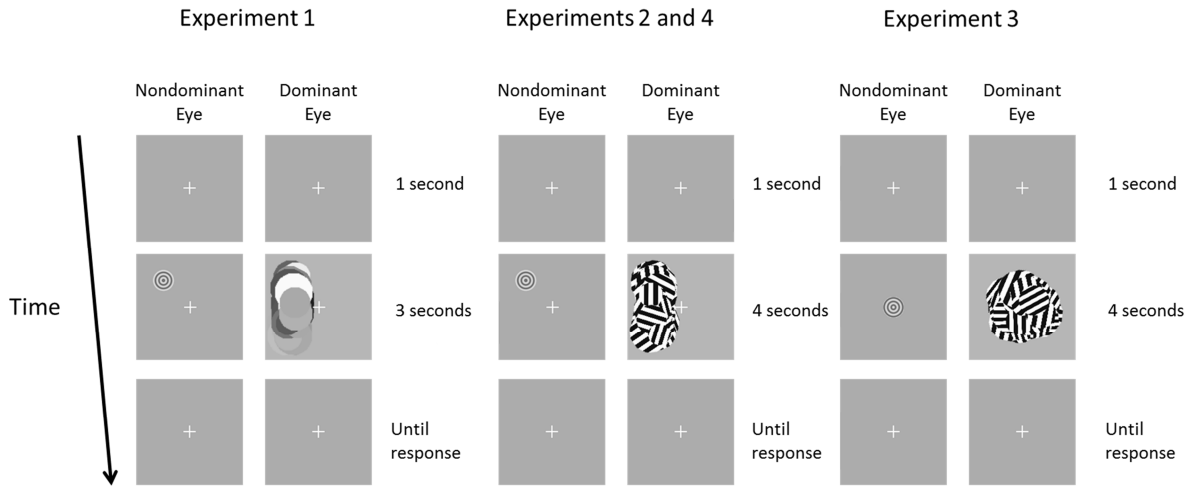

Figure 5.1. Trial sequence in all the experiments. A trial always started with 1 second of fixation after which the CFS mask and target stimulus (above or below fixation) were presented for 3 (Experiment 1) or 4 seconds (Experiments 2, 3, and 4). After stimulus presentation, the fixation display was presented until participants made their response.

Procedure and Design. Participants read written instructions before the experiment started. They were instructed that they could use the auditory stimulation to perform better on the task. This instruction was given because van Ee et al. (2009) reported that attending the auditory looming signal was critical for observing the multisensory facilitating effect on holding the visual looming stimulus in perceptual dominance. Participants were asked to fixate the fixation cross during the entire experiment. The most important instructions were verbally repeated. First, eye dominance was measured for each participant, based on the technique developed by Yang, Blake, and McDonald (2010). On each trial, an arrow which was gradually ramped up in contrast (100% after 2 seconds) was presented to one eye while the CFS mask was presented to the other eye, yielding initial perceptual suppression of the arrow. Upon breakthrough of the arrow, participants had to determine the direction of the arrow as fast as possible. In half of the trials, the arrow was presented to the left eye and in the other half to the right eye, for 80 trials in total (40 for each eye). The index for eye dominance was determined by calculating the ratio of the mean suppression time when the arrow was presented in the left eye and the mean suppression time when the arrow was presented in the right eye (Yang et al., 2010). After determining eye dominance, the CFS mask was always presented to the dominant eye during the rest of the experiment. A fifteen-

minute break was given after the eye-dominance measurement. The break was necessary to ensure the effectiveness of suppression on participants in the second part of the experiment.

In the second part, the instructions were repeated and – when ready – participants started the experiment. In the visual static condition (VS), only the concentric grating was presented to the non-dominant eye. In the visual looming condition (VL), the concentric grating was presented with looming motion. Lastly, in the audiovisual looming condition (AVL), the auditory tone pips were presented concurrently with the visual looming stimulus at the peak of each looming cycle, using a headset. Each condition consisted of 50 trials. On each trial, one of the conditions was randomly selected. A trial started with 1 second of fixation, after which the CFS mask and target stimulus (at the current contrast level of the staircase) were presented for 3 seconds. After 3 seconds, both disappeared and the participant had to respond at which spatial position – above or below fixation – the target stimulus had been presented (the spatial location of the target was randomly determined on each trial, but balanced across the experiment). Participants were instructed to guess if they had not seen the target during a trial. A QUEST staircase procedure was used to measure 75% contrast detection thresholds (Watson & Pelli, 1983) for each condition separately. The number of trials necessary for an accurate measurement of the detection threshold was based on a pilot study.

Results

Before subjecting the data to any statistical test, the thresholds were normalized to the mean threshold for each participant (computed across conditions). One participant was excluded from data-analysis due to suppression being too effective. For this participant the looming stimulus had to be presented at full contrast in all conditions in nearly all trials with performance levels still around chance (50%). In comparison, the average 75% contrast threshold across conditions and participants was 10%.

All analyses were done in a Bayesian framework, relying on Bayes Factors (BF) and 95% credible intervals (CI) on effect sizes. Calculation of BFs was done with the BayesFactor package (Rouder, Morey, Speckman, & Province, 2012). The models used to analyze the data are conceptually very similar, if not the same, to the classical repeated measures ANOVA. The advantages of statistical inference in a Bayesian compared to a frequentist framework

have been elaborately discussed elsewhere (Kruschke, 2010; Kruschke, 2010; Rouder, Speckman, Sun, Morey, & Iverson, 2009; Wagenmakers, 2007). With respect to this study, a major advantage of the use of BFs is that it quantifies the relative evidence for one model compared to another. Thus, and critical for our study, BFs can also be used to quantify the evidence for a null model (containing no effect of condition) compared to a model containing an effect of condition, which is impossible in standard null hypothesis significance testing. We use the classification proposed by Jeffreys (1961) as a guideline for interpreting the BFs (i.e., BFs > 3 or BFs < 1/3 will be considered substantial evidence for the one or the other model). 95% CIs were based on the posterior distribution of the effect size parameter of the model that was estimated (see Rouder et al., 2012, 2009).

Figure 5.2A depicts the mean normalized thresholds and the individual data for each subject. The BF analysis revealed that a condition effect was indeed present in the data (BF = 17). To disentangle the relative contribution of each condition to this effect, different contrasts were computed (summarized in Table 5.1). From Table 5.1, it is clear that the condition effect is driven by the difference between the AVL and VS condition. Although the AVL and VL conditions do not differ from each other, their combination does differ from the VS condition. This analysis is complemented by the 95% CIs which do not include zero for the AVL vs. VS and AVL/VL vs. VS contrasts.

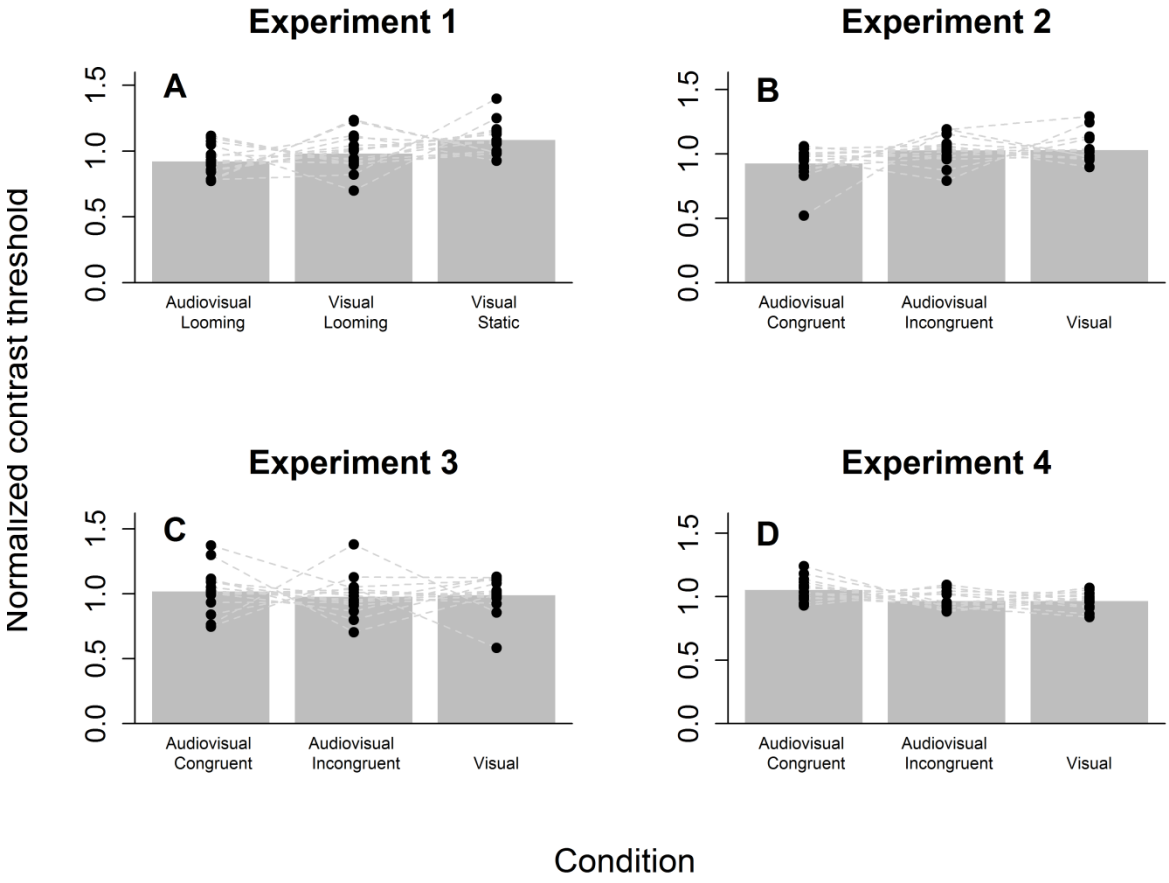

Figure 5.2. Summary of all four experiments. Bar plots depict the mean normalized contrast threshold for each condition. The dots indicate the normalized contrast thresholds for each participant. Connected dots refer to the same participant.

Table 5.1. Contrast analysis for Experiment 1.

Contrast	BF (relative to null)	Delta	95% CI
AVL vs. VL	0.41	-0.23	[-0.69; 0.22]
AVL vs. VS	15	-0.79	[-1.36; -0.23]
VL vs. VS	0.88	-0.37	[-0.89; 0.09]
AVL and VL vs. VS	6	-0.65	[-1.21; -0.12]

Note. Bayes Factors > 3 indicates substantial evidence *against* the null model. Delta refers to the mean posterior effect size. The 95% CI was calculated for this delta parameter.

Discussion

In Experiment 1, a strong effect was found for the combination of auditory stimulation and looming motion on detection thresholds compared to the static stimulus.

However, the results did not indicate that an audiovisual looming stimulus could be detected more easily - when initially suppressed from awareness - than a visual looming stimulus. Comparing both conditions that contained looming motion to the static stimulus indicated that the improvement in detection in the audiovisual looming condition seemed to be mostly driven by the looming motion characteristics of the target stimulus and was not driven by presenting auditory congruent stimulation together with the looming motion.

Because the average contrast at which participants detected the target was quite low (~10%), a floor effect might have obscured any, potentially bigger effects in a part of our sample. Therefore, we sought to improve the effectiveness of our CFS mask. Recently, some studies have reported evidence on the importance of feature similarity between the CFS mask and the suppressed stimulus to achieve effective suppression (Hong & Blake, 2009; Maehara, Huang, & Hess, 2009; Moors, Wagemans, & de-Wit, 2014; Yang & Blake, 2012). Therefore, we changed the properties of our mask to contain spatial frequency information matching that of the target stimulus.

Stimulus presentation during every trial amounted to 3 seconds in Experiment 1, and this might have been too short to allow for integration between the visual and auditory stimulus. Therefore, we increased the trial duration in the second experiment. The visual static condition was excluded, because the results of Experiment 1 indicated that the detection of the target stimulus in the static condition was reliably worse than in the audiovisual looming condition. An incongruent audiovisual condition was added to determine whether any facilitating effect of the audiovisual condition was due to generic auditory stimulation rather than multisensory integration of congruent signals.

EXPERIMENT 2

Methods

Participants. Fourteen of the same participants as in Experiment 1 and the two first authors took part in the second study. The participant that had to be excluded in the analysis of the first experiment was not invited to participate again. Two other participants did not wish to return to participate in the second study. The mean age of participants was 22 years (SD = 1.46). All participants signed an informed consent.

Apparatus and stimuli. The same settings were used as in Experiment 1 for the background, checkerboard pattern, fixation cross, and visual target stimulus. The CFS mask was adjusted to more optimally match the characteristics of the target stimulus. The new mask consisted of 50 circular square wave gratings with a randomly alternating size within a range of 1° and 2° (Figure 5.1). The spatial frequency of each element was randomly selected within a range of 2 to 4 cycles/degree and the orientation of each element ranged between 0 and π radians. In the audiovisual congruent condition, the tone pips were presented in the same way as in Experiment 1. In the audiovisual incongruent condition, the tone pips were rhythmically incongruent with the visual looming stimulus and presented at 0.8 Hz instead of 1 Hz as in the audiovisual congruent condition. All other settings of the experiment were kept the same as in Experiment 1.

Procedure and Design. Participants signed an informed consent and read written instructions. Since the majority of participants participated in Experiment 1, eye dominance did not need to be measured again for these participants. The visual looming (VL) and audiovisual congruent (AVC) conditions were the same as in Experiment 1. In the audiovisual incongruent (AVIC) condition, the tone pips were presented in a rhythmically incongruent fashion with the visual looming stimulus (i.e., at 0.8 Hz instead of 1 Hz). At the start of each trial, a fixation cross was presented for 1 second after which the target stimulus and CFS mask were presented for 4 seconds. After stimulus presentation, participants responded whether they saw the target stimulus above or below fixation. Target position was again randomly determined on each trial, but balanced across the experiment. The three different conditions – visual, audiovisual congruent and audiovisual incongruent – were presented in a random order for 50 trials per condition. 75% contrast detection thresholds were measured again using a QUEST staircase procedure (Watson & Pelli, 1983).

Results

Since the participants were partly the same as in Experiment 1, we could explicitly test whether the new CFS mask improved suppression compared to the one in Experiment 1. An improvement in average contrast was indeed observed (BF = 5; posterior mean effect size 0.51; 95% CI = [0.11; 0.91]). Figure 5.2B depicts the results of Experiment 2. The data were again analyzed using the normalized contrast thresholds (relative to the mean of each

participant across conditions). One subject was not included in the analysis because the mean contrast threshold deviated more than 3 standard deviations from the mean contrast threshold across all subjects. The BF analysis indicated no convincing evidence (BF = 2.11) for an effect of condition on the normalized contrast thresholds. The follow-up contrast analysis is summarized in Table 5.2. It is clear that the evidence for an effect of the AVC condition versus the other conditions is indecisive, i.e. both the null model and the model that indicates a difference between conditions are favored equally. Moreover, the results were strongly influenced by one participant with a normalized contrast threshold of approximately 0.5 in the AVL condition. Exclusion of this influential data point reversed the direction of the BF to weak evidence for the null model (BF = 0.68).

Table 5.2. Contrast analysis for Experiment 2.

Contrast	**BF (relative to null)**	**Delta**	**95% CI**
Including influential data point			
AVC vs. AVIC	1	-0.40	[-0.91;0.08]
AVC vs. VL	1	-0.40	[-0.92;0.1]
AVIC vs. VL	0.26	-0.02	[-0.48;0.44]
AVC vs. AVIC and VL	1.29	-0.45	[-.97;0.05]
Excluding influential data point			
AVC vs. AVIC	0.70	-0.35	[-0.88;0.14]
AVC vs. VL	0.85	-0.38	[-0.91;0.13]
AVIC vs. VL	0.27	0.02	[-0.47;0.50]
AVC vs. AVIC and VL	1.60	-0.50	[-1.05;0.02]

Discussion

In Experiment 2, again, no support was found for the hypothesis that the detection of the visual looming target would improve when presented in combination with congruent sound. In sum, in a sample nearly the same as in Experiment 1, we obtained, with an improved design, no evidence for audiovisual looming integration in the context of a subliminal visual looming stimulus. The estimates for the effect sizes for a difference

between the audiovisual congruent condition and one of the other conditions were about 0.40, generally considered to be a moderate effect according to the classification proposed by Cohen (1977). If we ignore the Proteus phenomenon (Button et al., 2013) and consider this an accurate estimate of the true effect, a power analysis for an effect size of 0.40 indicates that we should at least quadruple our sample to achieve 90% power.

Instead of quadrupling our sample, we decided to redesign our experiment such that, if multisensory integration is driving the direction of the differences observed in Experiments 1 and 2, these differences should be more pronounced in this experiment or, at least, the same. That is, van Ee et al. (2009) observed an attentional benefit of multisensory stimulation when the looming pattern was presented at fixation (similarly for the looming bias observed in Parker & Alais, 2007). Therefore, we presented the looming stimulus at fixation in Experiment 3. Furthermore, although tone pips were argued to be equally effective in van Ee et al. (2009), we implemented a continuous looming sound to increase the (in)congruency of the auditory and visual signals. That is, in Experiment 3, at every moment in time for the audiovisual conditions, either congruent or incongruent auditory signals were presented together with the visual signal. Last, to ensure that our effects were not specific to our previous sample of participants, we collected data from a new sample of participants.

EXPERIMENT 3

Methods

Participants. 16 new observers and one of the first authors participated in the experiment. All new observers participated in return for course credit. The mean age was 19 years (*SD* = 2.06). All participants signed an informed consent.

Apparatus and Stimuli. The visual target stimulus and CFS mask were identical to those in Experiment 2, but they were now presented at fixation. Instead of a tone pip, the auditory stimulus was a looming sound in the current experiment. A pure tone of 200 Hz was presented, periodically increasing and decreasing in amplitude. The increase in amplitude coincided with the phase-shifts of the visual looming motion (1 Hz) in the audiovisual congruent condition. In the audiovisual incongruent condition the frequency of the looming sound was 0.87 Hz. All other settings of the experiment were identical to Experiment 2.

Procedure and Design. The procedure was the same as in Experiments 1 and 2, except for the task participants had to perform. Since the target stimulus was always presented at fixation, participants had to indicate on each trial whether they had seen the target stimulus or not (yes/no classification) (Figure 5.1). The experiment consisted of the three same conditions as in Experiment 2 (visual looming, audiovisual congruent, and audiovisual incongruent). Catch trials (in which no target stimulus was presented) were included for all three conditions such that the absence or presence of sound would not be predictive for target presence. For each condition, we again measured contrast thresholds through a QUEST staircase procedure, for 50 trials per staircase. 50 catch trials were also included and at each trial it was determined randomly whether an "(in)congruent" sound would be presented during the catch trial or not. Thus, participants performed 200 trials in total. The different conditions were again randomized across trials.

Results

A mean accuracy level of 75% on the catch trials was the cut-off to include data. Two participants did not meet this criterion (8% and 63% accuracy). The mean accuracy of participants included in further analysis was 91.5% (SD = 7.3).

Figure 5.2C depicts the results of Experiment 3. The BF analysis was again done on the normalized contrast thresholds. As is already apparent from Figure 5.2C, the omnibus analysis did not indicate evidence for an effect of condition (BF = 0.20). Table 5.3 summarizes the contrast analysis. Here again, no evidence for an effect is found. To the contrary, the BFs for the omnibus analysis and contrasts indicate strong evidence in favour of the null model (i.e., all BFs < 0.33). Moreover, the direction of the differences between conditions seems to have reversed in Experiment 3 compared to Experiment 2. That is, numerically, the audio-visual congruent condition now had the highest mean normalized contrast detection threshold.

Table 5.3. Contrast analysis for Experiment 3.

Contrast	BF (relative to null)	Delta	95% CI
AVC vs. AVIC	0.30	0.11	[-0.35;0.58]
AVC vs. VL	0.28	0.09	[-0.37;0.56]
AVIC vs. VL	0.27	-0.04	[-0.51;0.42]
AVC vs. AVIC and VL	0.30	0.11	[-0.34;0.57]

Discussion

In Experiment 3, we tested a new sample of participants with a further refined design to maximize our chances of finding an effect of multisensory integration on detecting a suppressed visual looming stimulus. That is, we presented the looming stimulus at fixation and we used a continuously looming sound that was either congruent or incongruent with the visual looming stimulus. Again, no effects of (synchronous) auditory stimulation were observed. All BFs indicated strong evidence for the absence of an effect for any comparison between conditions that was considered.

To increase our confidence in this null effect, we decided to run Experiment 2 again on another sample of participants. We reasoned that if a true effect underlay the direction of the differences in Experiment 2, we should at least be able to replicate this pattern in a new sample of subjects, albeit the sample size not providing sufficient power. In contrast, if a null effect underlay the data of Experiment 2, we would expect that this new sample might show differences between conditions in different directions compared to Experiment 2. Crucially, however, analyzing the sample of Experiment 2 together with the new sample should cancel out any differences observed in both samples separately, if we are dealing with a null effect.

EXPERIMENT 4

Methods

Participants. 15 paid observers participated in the experiment. Their age ranged between 18 and 30 years. All observers provided informed consent before the start of the experiment.

Apparatus and Stimuli. The experimental set-up and stimuli were identical to Experiment 2.

Procedure and Design. The procedure and design were exactly the same as in Experiment 2.

Results and Discussion

Figure 5.2D depicts the results of Experiment 4. The data were again analyzed using the normalized contrast thresholds. Two participants were excluded from the analysis because at least one of their contrast thresholds estimated by the QUEST procedure exceeded the maximum contrast level. The BF analysis now indicated strong evidence in favor of an effect for condition (BF = 6). However, comparing these results with those obtained in Experiment 2, it becomes clear that the direction of the differences has now reversed. Thus, in a second analysis, we collapsed the data of Experiment 2 and Experiment 4 (Figure 5.3). The BF analysis now indicated substantial evidence for the null model (BF = 8). Thus, collecting additional data for Experiment 2 revealed that in the new sample an effect of condition was present (and stronger compared to Experiment 2, yet in a different direction), but an analysis of the aggregate data indicated substantial evidence for no differences between conditions.

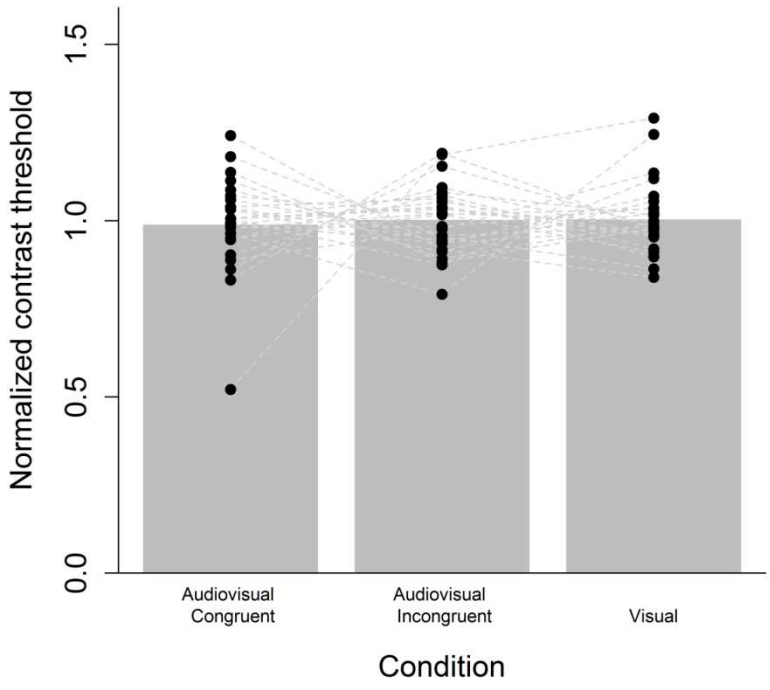

Figure 5.3. Mean normalized contrast thresholds collapsed for Experiments 2 and 4. The dots indicate the normalized contrast thresholds for each participant. Connected dots refer to the same participant.

GENERAL DISCUSSION

We examined whether an auditory signal can be integrated with a visual stimulus suppressed from awareness through Continuous Flash Suppression (CFS). To this end, we used a visual looming stimulus and concurrently presented tone pips (Experiments 1, 2, and 4) or a continuous looming sound (Experiment 3) and measured contrast detection thresholds of the visual looming stimulus. Multisensory integration between the supraliminal auditory stimulus and subliminal visual stimulus should lead to lower contrast detection thresholds in the congruent audio-visual condition compared to a unisensory visual condition or a multisensory condition in which the auditory stimulus was incongruent with the visual looming stimulus. In other words, due to multisensory integration, the strength of the representation of the suppressed visual stimulus would increase relative to the other conditions, effectively lowering suppression strength of the CFS mask and therefore requiring lower contrast to achieve the same performance. Across four experiments we obtained no evidence for audio-visual integration during interocular suppression. That is, the results were either equally supportive for either an effect of condition or no effect of condition (Experiments 1 and 2) or provided substantial evidence for the null model of no condition effect (Experiment 3, and Experiments 2 and 4 combined). Furthermore, in the contrast analyses, we never obtained convincing evidence for contrast thresholds being lower in the audio-visual congruent condition compared to the audio-visual incongruent or visual only conditions. Based on these results, the most parsimonious conclusion seems to be that, for the stimulus combinations we have used, integration between an auditory (looming) signal and a visual looming stimulus cannot be achieved in the absence of awareness of the visual stimulus (induced through interocular suppression), or at least, cannot bias breaking suppression in a manner consistent with predictions based on multisensory integration.

Our results thus seem to stand in apparent contrast with other studies using CFS to assess multisensory integration in the absence of awareness (Alsius & Munhall, 2013; Palmer & Ramsey, 2012; Plass et al., 2014; Salomon et al., 2013; Yang & Yeh, 2014; Zhou et al., 2010). The question thus remains as to why other studies have found an effect of multisensory integration for visual stimuli suppressed from awareness, when none was found here. Obviously, we are not in a position to resolve this apparent inconsistency immediately, but it

is worth noting a number of the core differences between different studies, particularly in terms of the types of methods and stimuli used.

Although the CFS paradigm provides unique opportunities to assess whether multisensory integration can occur in the absence of visual awareness (Deroy, Chen, & Spence, 2014), previous studies have often relied on breaking CFS (b-CFS) (Alsius & Munhall, 2013; Salomon et al., 2013; Yang & Yeh, 2014; Zhou et al., 2010). As already highlighted in the introduction, this approach is not without its critiques (Stein et al., 2011; Stein & Sterzer, 2014, see also a recent review by Gayet, Van Der Stigchel, and Paffen, 2014), one particular concern being that the observed difference in suppression time between conditions cannot be unambiguously attributed to processing differences during suppression. It might thus be that the absence of an effect in the current study reflects the use of a more stringent measure, being the contrast threshold at which something can be detected, rather than the time taken for a stimulus to break through suppression. Nevertheless, not all of these studies have relied on b-CFS, so this does not provide a full explanation for the discrepancy between the current study and previous studies.

It is interesting to note that both studies that have not used b-CFS to study multisensory integration for a suppressed visual stimulus were on the subject of audiovisual speech integration. For example, in the study of Palmer and Ramsey (2012) two visual lip-stream sequences were presented while suppressed through CFS and concurrently an auditory stream was presented, congruent with one of the visual lip-stream sequences. After presenting this sequence, the authors probed whether participants had allocated their attention to the spatial location at which the visual lip-stream sequence was either congruent or incongruent with the auditory stream. They did so by presenting a near threshold Gabor patch in one of the two locations, of which participants had to indicate the location and orientation. The results indicated that participants performed better on valid cue-target trials. Palmer and Ramsey (2012) argued that to be able to do this, the congruency relation between visual and auditory input has to be extracted, presumably through multisensory integration of both signals. A different study showed that participants respond faster to spoken words

when a concurrently presented face suppressed by CFS articulates that word compared to when the suppressed face articulates a different word (Plass et al., 2014).

Although our interest in testing whether auditory (looming) stimuli could be integrated with suppressed visual looming stimuli was partly motivated by the ecological and potentially evolutionary relevance of looming stimuli, it seems to be the case that, based on the results of these two studies, visual stimuli that are more naturalistic and potentially more relevant (ecologically and evolutionary) do seem to be integrated with supraliminal auditory stimuli. Indeed, the naturalistic structure of a stimulus has been shown to be an important determinant of audio-visual integration in binocular rivalry (Conrad et al., 2013). Furthermore, it should be noted that audio-visual speech stimuli provide more constraints as to which part of the auditory input should match which part of the visual input, which might be beneficial for integration when one of the inputs is rendered unconscious. Lastly, the results of these two studies particularly pertain to attentional orienting or response preparation in function of integrating subliminal visual and supraliminal auditory input. This type of integration might as well happen for the stimulus used in this study. Yet it could be that, due to the fact that the site of interocular suppression is usually located fairly early in the visual processing hierarchy (Blake & Logothetis, 2002; Logothetis, 1998), the representation of the suppressed stimulus is such that the synergistic effects observed for visible looming stimuli fail to play out in this situation (Cappe, Thelen, Romei, Thut, & Murray, 2012; Tyll et al., 2013).

If we broaden the scope of the discussion to also include studies that used regular binocular rivalry, it is interesting to note that most studies on audio-visual interactions in binocular rivalry observed an effect of the auditory stimulus that was restricted to an extension of dominance periods for the congruent visual stimulus (Chen et al., 2011; Conrad et al., 2010; Kang & Blake, 2005). In that respect, this study could be said to reveal a similar pattern, since there is no effect of auditory stimuli on contrast detection thresholds of suppressed visual looming stimuli. A remarkably different pattern emerges, however, if one considers the interaction between tactile and visual stimuli during binocular rivalry. Here, tactile stimuli do not only increase dominance durations of congruent visual stimuli, but also shorten suppression durations of these congruent visual stimuli (Lunghi & Alais, 2013;

Lunghi et al., 2010, 2014; Lunghi & Morrone, 2013). These findings seem to indicate a relatively early phase in the processing hierarchy at which these signals already interact, not only because the tactile stimuli can influence suppression durations, but also because these studies have shown that these effects are tuned to both orientation (Lunghi & Alais, 2013) and spatial frequency (Lunghi et al., 2010). On a speculative note, the difference between audiovisual and tactile-visual integration in binocular rivalry might be related to the level at which these stimuli are integrated. That is, neural processing related to an interocularly suppressed stimulus is mostly restricted to early visual areas (Blake & Logothetis, 2002; Logothetis, 1998). Given that evidence for integration between a suppressed visual stimulus and a tactile but not auditory stimulus is found, this could lead one to hypothesize that tactile-visual integration is possible in early visual areas (at least for the stimuli used in these studies) but that audio-visual integration requires a contribution from higher areas. Indeed, studies have consistently reported an important role for the superior temporal sulcus in audio-visual integration (Stevenson, Geoghegan, & James, 2007; Stevenson & James, 2009; Tyll et al., 2013). Suppressing a visual stimulus through interocular suppression might thus block the feed forward progression of visual input to a stage in the system where audio-visual integration can exert its synergistic effects.

A notable exception to this general pattern is the recent study of Lunghi et al. (2014). In this study, the authors demonstrated not only an increased probability of maintaining the current percept when it was congruent with an auditory or tactile stimulus, but also an increased probability of switching to the other stimulus when the current percept was incongruent with the tactile or auditory stimulus. As such, Lunghi et al. (2014) provided evidence that both auditory and tactile stimuli can be integrated with suppressed visual stimuli during binocular rivalry. It should be noted, however, that this study relied on temporal frequency rivalry (Alais & Parker, 2012). Although temporal frequency rivalry resembles spatial rivalry with respect to dominance duration distributions and alternation rates, the mechanisms through which it acts might as well allow for integration between a supraliminal auditory or tactile stimulus and a suppressed visual stimulus whereas spatial rivalry might not. Lastly, compared to our study, Lunghi et al. (2014) tracked dominance durations, whereas we were interested in potential integration at the first stage of suppression. Taking together these findings, one might thus speculate that integration

between a supraliminal auditory or tactile stimulus and suppressed visual stimulus only plays out in the later phases of stimulus presentation, after a few alternations between stimuli have occurred.

CONCLUSION

This study sought to address whether multisensory integration between an auditory stimulus and a visual looming stimulus can be achieved when the looming stimulus is presented in the absence of awareness induced through CFS. In four experiments, contrast detection thresholds of the visual looming stimulus were measured in a static (Experiment 1), visual looming (Experiments 1, 2, 3 and 4), audio-visual congruent (Experiments 1, 2, 3 and 4) and audio-visual incongruent (Experiments 2, 3 and 4) condition. The accumulated evidence from these four studies suggests that congruent audio-visual signals are not able to reduce the contrast detection threshold at which visual stimuli can be detected. Thus, for audio-visual looming stimuli, no evidence for multisensory integration in the absence of visual awareness was found. Although these results are in line with the general pattern of results on audio-visual interactions in binocular rivalry (but see Lunghi et al., 2014; Palmer & Ramsey, 2012; Plass et al., 2014), it is currently unclear whether differences with previous studies reflect methodological differences in the measurement techniques used (contrast detection thresholds, b-CFS, or priming), in the modalities tested (auditory vs. tactile) or the types of stimuli used (abstract vs. naturalistic).

ACKNOWLEDGMENTS

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Chapter 6.
Launching awareness: causal events enter awareness faster than non-causal events

Philosophers have long argued that causality cannot be directly observed, but that causality has to be inferred (Hume, 1738). Albert Michotte however developed numerous visual phenomena in which people seemed to perceive causality as a primary visual property (like color or motion), tightly coupled to specific spatiotemporal stimulus properties and apparently not requiring a conscious cognitive inference (Michotte, 1946). Over the last seventy years, advocates of both sides have continued this debate and no consensus has been reached (Rips, 2011; Scholl & Tremoulet, 2000; Wagemans, van Lier, & Scholl, 2006; Weir, 1978; Wolff, 2007). Using a continuous flash suppression (CFS) paradigm, we show that causal events enter awareness faster than non-causal events. We presented observers with 'causal' and 'non-causal' events, that participants were not immediately aware of, and found consistent evidence that participants become aware of causal events more rapidly than of non-causal events. Since CFS disrupts neural activity beyond early visual areas, our results suggest that, whilst causality must be inferred from sensory evidence, this inference is computed at low levels of perceptual processing, and does not depend on a deliberative conscious inference. This work therefore supports Michotte's contention that, like color or motion, causality is an immediate property of our perception of the world.

Moors, P., Wagemans, J., & de-Wit, L. (in preparation). Launching awareness: causal events enter awareness faster than non-causal events.

INTRODUCTION

Philosophers and psychologists alike have long debated whether causality can be directly perceived or requires a conscious cognitive inference (Hume, 1738; Michotte, 1946). In the 1940s, Albert Michotte provided an important contribution to this discussion by demonstrating that parametric variations in the spatiotemporal attributes of a stimulus could predictably influence whether a causal percept was elicited. Based on this series of experiments, Michotte argued that human observers *perceive* causality, and that causality is a primary visual property such as color or motion. In one classic example, Michotte presented observers with a *launching* event in which an object moves toward another stationary object, stops right in front of it, and then the second object immediately starts to move along the same trajectory (Figure 6.1a). This event almost irresistibly evokes a causal impression, in which the first object appears to cause the motion of the second object. In the seventy years following his seminal contribution, the discussion on the perceptual versus cognitive basis of causality perception has remained wide open (Arnold, Petrie, Gallagher, & Yarrow, 2015; Rips, 2011; Rolfs et al., 2013; Scholl & Tremoulet, 2000; Wagemans et al., 2006; Weir, 1978; Wolff, 2007). In this work, we attempted to address this issue by asking whether conscious processing of causal and non-causal events is necessary for them to be differentially processed. To render causal and non-causal events invisible, we used a modified version of continuous flash suppression (CFS) (Moors, Wagemans, & de-Wit, 2014; Tsuchiya & Koch, 2005) that was able to perceptually suppress moving events for extended periods of time. CFS is a potent technique for suppressing stimuli presented to one eye by presenting a dynamic noise pattern to the other eye. Furthermore, CFS is known to disrupt neural activity beyond early visual areas (Hesselmann & Malach, 2011; Yuval-Greenberg & Heeger, 2013). Hence, if a differential sensitivity to causal and non-causal events is revealed through CFS, this implies that the visual system processes these events at a very early stage during visual processing.

In our first experiment, we presented observers (n = 24) with launching (causal) and passing (non-causal) events while these were rendered invisible through CFS. In the launching event, a disc would start moving toward a stationary disc, stop right in front of it, after which the stationary disc would start moving. The passing event was the same, except

that the first disc completely overlapped the second disc before the latter started moving (Figure 6.1a). These events were continuously repeated until observers detected any aspect of the discs emerging through the CFS mask. Importantly, we never informed observers on the nature of the events. Suppression time was then taken as a measure of the extent to which the events were processed during perceptual suppression (Gayet, Paffen, & Van der Stigchel, 2013; Jiang et al., 2007; Stein, Hebart, et al., 2011). Our results indicated that launch events entered awareness faster than pass events (BF > 100), for nearly every observer (Figure 6.1c). In our second experiment (n = 27), we sought to replicate this effect whilst including an event that controlled for differences in local motion saliency between the launch and pass event. The control event (pseudo-launch) was exactly the same as the launch event, except for the starting position of the first disc, which was shifted such that it would stop after passing by the side of the second stationary disc. Thus, the secondary disc would only start moving after the first disc had stopped in a location where it could not logically have caused the motion of the second (Figure 6.1a). The results of this second experiment replicate the result of the first study showing that launch events broke suppression faster than pass events (BF > 100). Critically, launch events also broke suppression faster than pseudo-launch events (BF = 54), and pass and pseudo-launch events broke suppression equally fast (BF < 0.05).

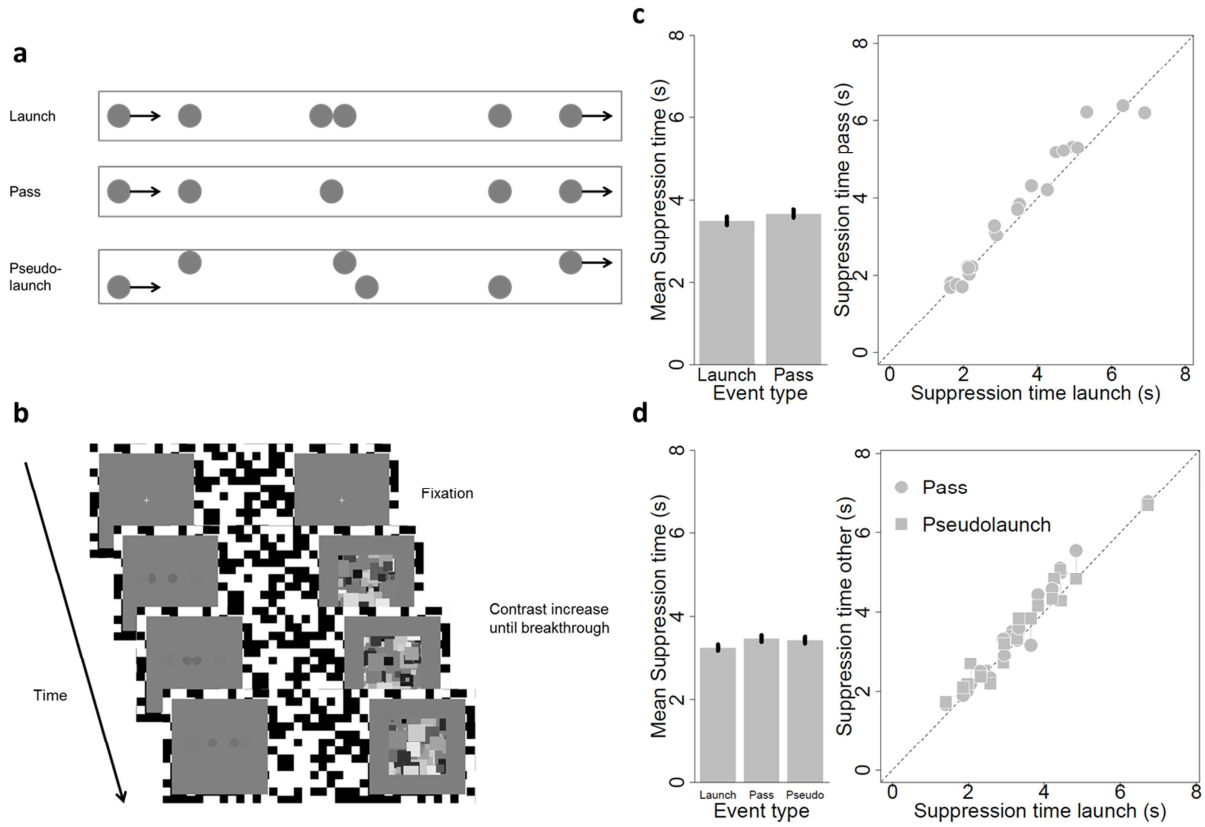

Figure 6.1. Study overview. (a) Events used in both experiments. In the launch event, a disc starts moving toward a stationary disc, stops right in front of it, and the stationary disc then starts moving. In the pass event, a disc starts moving toward a stationary disc, stops when it fully overlaps with the stationary disc, and the stationary disc then starts moving. In the pseudo-launch event, a disc starts moving toward a stationary disc, and stops after passing by the side of the stationary disc, after which the stationary disc starts moving. (b) Trial sequence used in the experiment. After a fixation period (1 second), the moving CFS mask was presented to the dominant eye and the (non-) causal event to the other eye. The disc events repeated and gradually increased in contrast until observers detected any part of the discs. (c) Results of Experiment 1. Launch event enter awareness faster than pass events. Almost all data points lie above the identity line. (d) Results of Experiment 2. Launch events enter awareness faster than pass or pseudo-launch events and pass and pseudo-launch events do not differ. Again, nearly all data points lie above the identity line.

Reflecting back on the philosophical debate regarding the perception of causality, the claim from Hume (1738) that *"… we are never sensible of any connexion betwixt causes and effects"* may be true on one level, but only to the extent that our senses have no direct access

to any physical properties. When we sense motion, color or shape we are only ever aware of a perceptual inference made by our visual system (Helmholtz, Gregory, Hoffman). In this sense, just as we do not have to consciously infer that movement has occurred, or that an apple is round or red, so too this result suggests that the distinction between causal and non-causal events is computed without a conscious inference. Michotte's (1946) previous demonstrations suggested that this was the case by highlighting that systematic parametric variations influenced the perception of causality in a way that would not obviously be predicted if this perception was dependent upon a conscious inference. Participants in Michotte's experiments were however fully aware of the stimuli being presented, and were primed to think about, and make judgements in terms of causality, thus it was always impossible to rule out that participants were in some way influenced to adopt a strategy that biased their perception, or at least their perceptual reports. In this experiment however, the distinction between causal and non-causal events influenced participants behavior in a way that could not have been shaped by a conscious bias towards one stimulus or another. Our results suggest that the distinction between causal and non-causal events must be inferred at early stages of visual processing, and that Michotte was correct in asserting that *"there is actual perception of causality, in the same sense that there is perception of shapes, movements (il y a veritablement perception de la causalité, au meme titre qu'il y a perception de formes, de mouvements)."* In sum, privileged access to awareness for causal events provides strong evidence for a perceptual account of causality perception, suggesting that causality, like shape or motion, is a basic feature of our experience of the world.

METHODS

Participants.

We recruited 24 participants in Experiment 1 and 26 participants in Experiment 2. All participants had normal or corrected-to-normal vision and participated in return for course credit or a monetary compensation. Both experiments were approved by the local ethics committee of the KU Leuven. All participants provided written informed consent prior to the start of the experiment.

Apparatus.

Stimuli were shown on two 19.8-in. Sony Trinitron GDM F500-R (2048 x 1536 pixels at 60 Hz, for each) monitors driven by a DELL Precision T3400 computer with an Intel Core Quad CPU Q9300 2.5 GHz processor running on Windows XP. Binocular presentation was achieved by a custom made stereo set-up. Two CRT monitors, which stood opposite to each other (distance of 220 cm), projected to the left and right eye respectively via two mirrors placed at a distance of 110 cm from the screen. A head- and chin rest (15 cm from the mirrors) was used to stabilize fixation. The effective viewing distance was 125 cm. Stimulus presentation, timing and keyboard responses were controlled with custom software programmed in Python using the PsychoPy library(Peirce, 2007, 2009). The experimental set-up was the same for both experiments.

Stimuli.

The background of the display in both eyes consisted of a random checkerboard pattern to achieve stable binocular fusion. The individual elements of the checkerboard were 0.34° by 0.34°. In both eyes, a gray frame (10° by 10°) where the stimuli were presented was superimposed on the checkerboard pattern. A black (eye dominance measurement) or red (main experiment) fixation cross was continuously present during the experiment (size 0.5° by 0.5°). In the eye dominance measurement phase, the target consisted of an arrow (maximal width 4°, maximal height 2°) and the CFS mask consisted of 150 squares with randomly selected sizes between 1° and 2° and a random luminance value. Two gray discs (1° of visual angle) were used to generate the launch, pass (Experiment 1), and pseudo-launch (Experiment 2) events. The starting position of the first disc was located at one of 8 different positions along a virtual circle (2.5° radius) at equally spaced angles (0 – 270°, in steps of 45°). The starting position of the second disc was always in the center of the virtual circle. The center of the virtual circle was jittered on each trial (in a range of +- 0.5° horizontally and vertically). The discs moved at a speed of 5°/s. The first disc always appeared from behind a virtual occluder and the second disc would disappear behind a virtual occluder as the event sequence ended. After 100 ms, the event initiated again in the reverse order and this event loop would continue until the participant's response. In the case of a launch, the first disc always stopped just before the second disc after which the second

disc started moving. For a pass event, the first disc stopped when it fully overlapped with the second disc after which the second disc started moving. The pseudo-launch event was modified from the sequence of the launch event. A pseudo-launch event was generated by shifting the starting position of the first disc upward or downward (relative to the second disc, orthogonal to the direction of motion of the disc) by half the size of the disc. The starting position of the first disc was also shifted forwards 1.5 times the size of the disc in the direction in which it would travel, such that during its movement it would pass next to the second disc, and stop just after it had passed by the side of the second disc. The second disc would then move in exactly the same manner in which it moves in the launch event. The CFS mask (8° x 8°) consisted of 104 moving squares(Moors et al., 2014) with a random luminance value and size (uniform range 0.2 to 2°), moving at a speed between 3°/s and 7°/s (randomly determined for each element using a uniform range). The mask elements would be partially occluded if they moved outside of the 10° x 10° frame. If they disappeared completely from behind one side of the frame, they would be moved behind the opposite side of the frame continuing on the same trajectory.

Procedure.

In the first part of the experiment, participants' eye dominance was measured according to the procedure outlined by Yang et al.(E. Yang, Blake, et al., 2010). Here, on each trial, a CFS mask was presented to one eye while an arrow pointing either to the left or the right was presented to the other eye and gradually increased in contrast. Upon breakthrough of the arrow stimulus, the participant had to indicate its direction as quickly as possible. Eye dominance was then determined by comparing the mean suppression times for both eyes, and the eye for which the CFS mask elicited the longest mean suppression times was taken as the dominant eye. In all subsequent parts of the experiment, the CFS mask was presented to this eye.

In the main part of the experiment, participants were presented on each trial with a moving CFS mask in one eye and a launch or pass event in the other eye (presented in one of eight directions). The discs started at 3% contrast at the beginning of the trial and the contrast was increased by 3% after each iteration of the event sequence. The participants' task was to indicate as quickly as possible when they perceived one or more discs that

became visible among the moving squares. We included catch trials in which no event was presented. These catch trials self-terminated after 10 seconds, upon which a new trial was initiated. Participants were told before the start of the experiment that trials could sometimes self-terminate, and that this was a characteristic of the experiment to ensure trials would not take too long to complete. Before starting the main experiment, participants completed a practice block to become acquainted with the task.

Design.

The experiment consisted of a 2 x 8 within-subjects design with the factors event type (launch vs. pass) and event direction (eight different, evenly spaced, directions). Participants completed 192 experimental trials in total. 48 catch trials were included. All conditions were randomized across trials. The practice block consisted of 16 trials.

Data-analysis.

All participants that responded on more than 10% of catch trials were removed from the data (Experiment 1: n = 2; Experiment 2: n = 5). Furthermore, because we wanted to analyze suppression times that were recorded after the first appearance of an event (which was after 500 ms), we excluded all trials in which the suppression time was shorter than 1 second. This ranged from deleting no trials to 38% of trials (Experiment 1: M = 7.6, SD = 12; Experiment 2: M = 6.5, SD = 10.5). Please note that including these trials, or including the participants that did not perform well on the catch trials, does not change the outcome of the analyses. Suppression times were logarithmically transformed due to their positive skewness, before subjecting them to any analysis. All analyses were conducted in R 3.2.0, a statistical programming language (R Core Team, 2014). All statistical tests were performed in the Bayesian framework relying on model selection through Bayes Factors, using the R BayesFactor package (version 0.9.11-1; Morey & Rouder, 2015). The Bayes Factor quantifies the relative degree of evidence of one statistical model versus another and therefore provides an intuitive measure to quantify the degree of belief in one statistical model over another (e.g., a model with and without a main effect of event type). All fitted models were ANOVA style models including random intercepts for participants, and the default settings for the priors (medium prior scale for fixed effects and nuisance prior scale for the participant

effect). As a guideline to interpret the resulting Bayes Factors, we use the classification proposed by Jeffreys (1961) in that Bayes Factors from 3 onwards constitute substantial evidence for one model over the other. All reported Bayes Factors can be interpreted as how much less likely a model is compared to the best fitting model.

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AUTHOR CONTRIBUTIONS

L.D. conceived of the experiment, and all authors contributed to the design. P.M. performed the experiments and analyzed the data. P.M. wrote the paper and critical revisions were provided by all authors.

Chapter 7.
Motion coherence, not body inversion affects the entry of point-light walkers into visual awareness during continuous flash suppression

In order to learn more about which kinds of perceptual processing are possible when stimuli are not consciously perceived, we investigated breakthrough times of a configural biological motion stimulus with continuous flash suppression (CFS). That is, we asked whether inverting a biological motion stimulus would differentially affect the time it takes to enter visual awareness. This question was motivated by the consistent finding of inversion effects for static face and body stimuli. Moreover, similar neural regions are active during face and body perception and the perception of biological motion. However, our results revealed no effect of stimulus inversion on breakthrough times, although motion coherence of the point-light figures did affect suppression times such that scrambled point-light figures broke suppression slower than coherent ones. These results indicate that a limited form of spatiotemporal integration can take place during CFS, yet that this integration does not profit from the properties that are specific to the higher-level meaningful Gestalts of the point-light figure.

Moors, P., Wagemans, J., & van de Cruys, S. (under review). Motion coherence, not body inversion affects the entry of point-light walkers into visual awareness during continuous flash suppression.

INTRODUCTION

The extent to which perceptual stimuli are processed automatically and without awareness is a topic of enduring interest in cognitive science (Dehaene & Changeux, 2011). For example, can local elements be grouped into a coherent form without being consciously perceived (Schwarzkopf & Rees, 2015)? If so, does this extend to global objects with salient meanings? Might unconscious processing even be selectively sensitive to some object categories, as sometimes suggested, for example based on their familiarity or behavioral relevance (e.g., faces)? In our current contribution to these issues, we used a biological motion stimulus, an easily manipulable, dynamic and meaningful configuration that relies on local and global integration of multiple elements across time and space.

As our method for rendering visual stimuli invisible (Kim & Blake, 2005), we used continuous flash suppression (CFS), in which a dynamic flashing pattern consisting of shapes of various colors is presented to one of the observer's eyes while a static stimulus is presented to the other (Tsuchiya & Koch, 2005). The flashing pattern renders the static stimulus invisible to the observer in the order of several seconds, enabling the researcher to study the extent to which the suppressed stimulus is still processed. One of the ways in which CFS has often been implemented is by measuring the time it takes for the suppressed stimulus to become visible to the observer (referred to as the breaking CFS paradigm; b-CFS). The reasoning behind this paradigm is that stronger stimuli will break suppression faster, suggesting privileged processing during interocular suppression. The use of b-CFS has generated a rich literature on the degree to which various stimuli break suppression differentially (for an overview, see http://gestaltrevision.be/s/cfs).

One of the most consistent findings that has been obtained using the b-CFS paradigm is that upright faces break suppression faster than inverted faces (Jiang et al., 2007; Stein, Hebart, et al., 2011). The effect resembles the well-known face inversion effect (FIE) for fully visible face stimuli (Farah, Tanaka, & Drain, 1995; Yovel & Kanwisher, 2005), even though this concerns discrimination performance rather than mere detection. The fact that a FIE can be obtained in b-CFS suggests that mechanisms related to face processing might still be preserved during interocular suppression. This is supported by some neuroimaging studies showing that differential BOLD activity can be observed for perceptually suppressed face stimuli, varying in their emotional expression (Jiang & He, 2006). Moreover, using multi-

voxel pattern analysis, Sterzer, Haynes, and Rees (2008) showed that it is possible to decode from the fusiform face area whether a perceptually suppressed face or house stimulus had been presented.

A recent study by Stein, Sterzer, and Peelen (2012) showed that not only faces but also whole bodies show an inversion effect. That is, headless bodies, human silhouettes, and variable body postures all consistently showed inversion effects in breaking suppression, yet this was not the case for inanimate objects. From this, Stein et al. concluded that the processes that mediate the time it takes for stimuli to break suppression are particularly sensitive to conspecifics (bodies and faces of other animals showed considerably less inversion effects). Again, the inversion effect observed for body postures in general resembles the body inversion effect (BIE; Reed, Stone, Bozova, & Tanaka, 2003) that is observed for consciously visible body stimuli (again concerning improved discrimination for upright bodies). Interestingly, this BIE seems mediated by face-selective, rather than body-selective, brain regions (Brandman & Yovel, 2010), suggesting both BIE and FIE rely on (partly) similar mechanisms.

The effortlessness with which human observers can recognize their conspecifics' behavior even in the case of impoverished perceptual input is clearly exemplified by point-light figures (Johansson, 1973). These stimuli are generated by placing dots on the joints of a human body. When statically presented, this stimulus is perceived as a meaningless group of dots, but once set in motion, human observers swiftly organize the dots into a meaningful whole and perceive a conspecific performing a certain action (hence, biological motion) rather than a set of independently moving dots. A substantial amount of research has addressed the extent to which intentions, emotions, gender, etc. can be derived from point-light figures (e.g., Blake & Shiffrar, 2007; Pollick, Lestou, Ryu, & Cho, 2002). Moreover, it has been shown that the detection of biological motion develops early in life (Simion, Regolin, & Bulf, 2008) and is robust against noise in the input (Neri, Morrone, & Burr, 1998).

As is the case for face and body stimuli, inversion of the point-light figure impairs the perception of biological motion, across experimental tasks (Bertenthal & Pinto, 1994; Pavlova & Sokolov, 2000). Similar to the FIE, the BIE in biological motion has been attributed to high-level configural processing (which seems greatly diminished by inversion), although the

local motion profile in the feet region also plays a role (Chang & Troje, 2009; Troje & Westhoff, 2006).

Combining the evidence for privileged processing of faces and bodies in b-CFS with the shared mechanisms through which faces and bodies and biological motion stimuli are being processed, raises the question whether it would also be possible to observe a body inversion effect for perceptually suppressed biological motion stimuli. This would indicate that the separate collection of dots can be integrated into a meaningful whole during interocular suppression, which can subsequently bias the interocular competition process to facilitate access to awareness for biological motion stimuli. In addition, to ensure that we could show that motion information was indeed extracted during interocular suppression, we manipulated motion coherence (coherent vs. scrambled) of the point-light figure. Indeed, previous studies have shown that coherently moving stimuli break suppression faster than randomly moving stimuli (Chung & Khuu, 2014; L. Kaunitz et al., 2013).

If the dots comprising the biological motion stimulus indeed are integrated during interocular suppression, we predicted to observe a body inversion effect for the coherent condition, yet none for the scrambled condition. Furthermore, based on the observations of Kaunitz et al. (2013) and Chung and Khuu (2014), we expected that scrambled stimuli would break suppression more slowly than coherently moving stimuli.

METHODS

Participants

17 people participated in the experiment for a monetary compensation. All participants had normal or corrected-to-normal vision and were naïve with respect to the purposes of the experiment. Every participants signed an informed consent before the start of the experiment. The experiment was approved by the local ethics committee of the faculty.

Apparatus

Stimuli were shown on two 19.8-in. Sony Trinitron GDM F500-R (2048 x 1536 pixels at 60 Hz, for each) monitors driven by a DELL Precision T3400 computer with an Intel Core Quad CPU Q9300 2.5 GHz processor running on Windows XP. Binocular presentation was achieved by a custom made stereo set-up. Two CRT monitors, which stood opposite to each

other (distance of 220 cm), projected to the left and right eye respectively via two mirrors placed at a distance of 110 cm from the screen. A head- and chin rest (15 cm from the mirrors) was used to stabilize fixation. The effective viewing distance was 125 cm. Stimulus presentation, timing and keyboard responses were controlled with custom software programmed in Python using the PsychoPy library (Peirce, 2007, 2009).

Stimuli

A random checkerboard pattern consisting of white and black square elements (0.34° x 0.34°) was used as a background and presented to both eyes to achieve stable binocular fusion. A gray square frame was superimposed on the checkerboard pattern to present all stimuli (10° x 10°). A white fixation cross was continuously present throughout the experiment (0.5 ° x 0.5°). In the eye dominance phase, an arrow was used as a target stimulus (maximal width 4°, maximal height 2°) and the CFS mask consisted of 150 squares with randomly picked sizes between 1° and 2° and a random grayscale value.

The point-light walker stimuli were adapted from previous studies (Schouten & Verfaillie, 2010; Van de Cruys, Schouten, & Wagemans, 2013). 15 small white dots (size 0.2°) were placed on coordinates from (Troje, 2002) (size 2°). The point-light walker moved at a speed of 2°/s. Scrambled versions of the walkers were generated by simultaneously phase and position scrambling. Phase scrambling was achieved by taking a random starting location for each element trajectory. Position scrambling was achieved by randomly displacing the position of each dot between 0 and 1 standard deviation up/downwards and left/rightwards from the original x- and y-coordinates. Because feature similarity between mask and stimulus has been shown to be important for effective suppression (Hong & Blake, 2009; Maehara et al., 2009; P. Moors et al., 2014; E. Yang & Blake, 2012), the CFS mask consisted of 400 grayscale discs with varying sizes between 0.2° and 1.2°. In both phases of the experiment, the rate at which the CFS mask refreshed its contents was set at 10 Hz.

Procedure

First, eye dominance was measured using the procedure of Yang, Blake, and McDonald (2010). On every trial, a fixation cross was presented for 1 second. Next, an arrow was presented in one eye and the CFS mask in the other. Participants had to indicate the

direction of an arrow as soon as it broke through the CFS mask by pressing “1” (for left) or “3” (for right) on a numerical keyboard. The CFS mask was randomly presented to the left or right eye (40 trials per eye) and after 80 trials eye dominance was determined as the eye in which the mean suppression time of the arrow was lowest. In all subsequent parts of the experiment, the CFS mask was presented to the participant's dominant eye.

In the main part of the experiment, participants were presented on each trial with a (biological) motion stimulus located either to the left or right of the fixation cross. The contrast of the motion stimulus increased linearly from 0 to 30% over 4.25 seconds and remained fixed at 30% until the participant's response. Upon breakthrough of the stimulus, participants had to indicate the stimulus location (left/right) as quickly as possible by means of a button press. Every trial started with a 1 second fixation period after which the motion stimulus was presented to the non-dominant eye and the CFS mask to the dominant eye. Participants first completed a practice block to get acquainted with the task before they started the main part of the experiment.

Design

The experiment consisted of a 2x2 full-factorial within-subjects design with stimulus inversion (upright vs. inverted) and stimulus scrambling (coherent vs. scrambled) being the factors manipulated. Every condition was tested 80 times, resulting in 320 trials in total. All conditions were presented in a fully random order. Participants could take a break after every fourth of the trials was completed. Before starting the main experiment, participants completed 8 practice trials.

RESULTS

Suppression times were log transformed due to their positive skew and only correct responses were included in the analysis. Outliers were defined as being higher than the mean plus three times the standard deviation of the suppression times and these were also removed from the data set (for each observer separately). This resulted in the removal of 2.5% of the data in total. The mean suppression times for each condition are depicted in Figure 7.1.

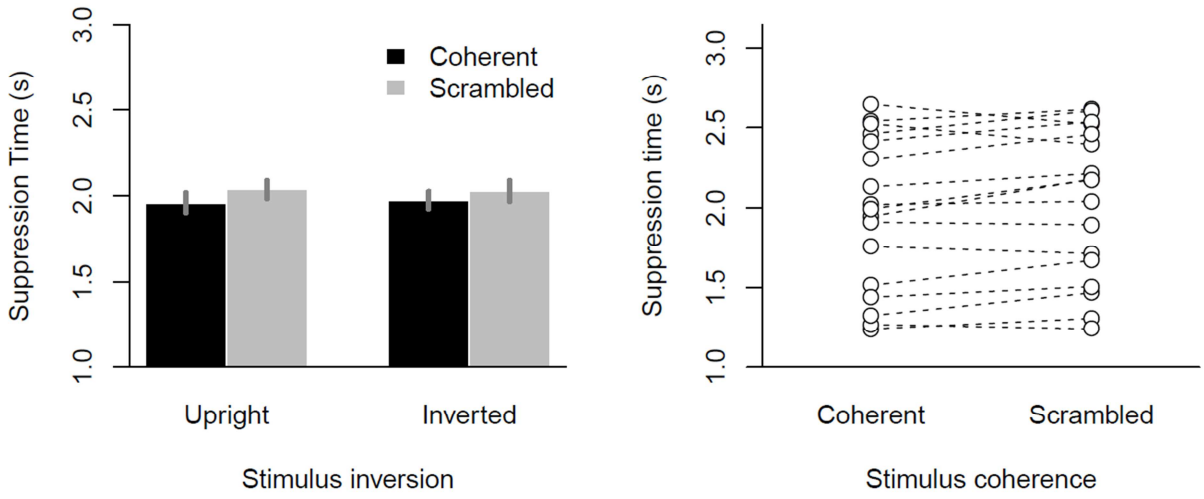

Figure 7.1. (left) Mean suppression times for all different conditions. Error bars denote 95% within-subject confidence intervals according to Morey (2008). (right) Motion coherence effect for each observer. Connected dots refer to the same participant.

All statistical analyses were performed in a Bayesian framework relying on a model selection approach through Bayes Factors. The Bayes Factor quantifies the relative degree of evidence of one statistical model over another and therefore provides an intuitive measure to quantify the degree of belief in one statistical model over another (e.g., a model with and without a main effect of stimulus type). The R package BayesFactor (version 0.9.9) was used to compute the Bayes Factors (Morey & Rouder, 2015). All considered models were ANOVA-style models including random intercepts for participants, and the default settings for the priors (medium prior scale for fixed effects and nuisance prior scale for the participant effect). As a guideline to interpret the resulting Bayes Factors, we use the classification proposed by Jeffreys (1961) in that Bayes Factors from 3 onwards constitute substantial evidence for one model over the other. All reported Bayes Factors can be interpreted as how much more likely the best fitting model is relative to the model under consideration. For example, a Bayes Factor of 3 can be interpreted as the best model being three times more likely than the model under consideration. The results of the Bayes Factor analysis are summarized in Table 7.1. As is apparent from this analysis, the model with only a main effect of scrambling was preferred strongly over a model with no effects or with both main effects of scrambling and stimulus inversion.

Table 7.1. Bayes Factor analysis.

Model	Bayes Factor
Scrambling	1
No effect	12.7
Scrambling + Inversion	28.7
All other models	> 100

Note. All Bayes Factors can be interpreted relative to the best fitting model (for which the Bayes Factor is 1). Bayes Factors > 1 can be interpreted as being that many times less likely as the best fitting model.

DISCUSSION

The goal of this study was to assess whether perceptually suppressed moving point-light figures would break CFS differentially when they were presented upright or inverted. This question was motivated by the fact that face and body inversion effects have been observed in a b-CFS context before, yet relying on static rather than dynamic stimuli. Furthermore, studies have shown that processing of biological motion stimuli and face and body stimuli share similar neural regions. Privileged access to awareness for upright rather than inverted point-light figures could thus indicate that the separate elements are integrated into a global, coherent whole during perceptual suppression. Our data reveal that, contrary to what is reported for static faces and bodies, inverted moving point-light figures do not break CFS any later than upright ones. What does impact suppression times, according to our findings and in line with the observations of Kaunitz et al. (2013) and Chung and Khuu (2014), is spatiotemporal coherence, since phase- and position-scrambled point-light walkers were, on average, slower to break suppression. The latter finding indicates that motion information indeed was extracted during interocular suppression.

These findings show that the inversion effect for static (grayscale) images of bodies as reported by Stein et al. (2012) does not generalize to dynamic, point-light figures. Importantly, this is also contrary to the (static) face inversion effect, one of the most robust findings in the b-CFS literature (Jiang et al., 2007; Stein, Hebart, et al., 2011). There are at least two ways to interpret this. The first is to question the power of inversion in eliminating all the characteristic configural biological motion perception. This implies that the difference

between inverted and upright is smaller than usually assumed. Indeed, while the interpretation of biological motion is greatly diminished by inversion, the superior temporal sulcus, one of the key brain regions responsive to biological motion, is still activated by inverted point-light figures similarly to intact figures and more than for scrambled figures (Grossman & Blake, 2001; Thompson, Clarke, Stewart, & Puce, 2005). However, virtually any behavioral task performed on point light figures shows clear inversion effects, so it is reasonable to assume this to be the case under CFS as well.

The second interpretation has to do with the type of processing that is possible under CFS. Apparently, although global motion coherence was extracted, the single elements of the point-light figure were not integrated such that they were represented as a meaningful, coherent whole. The precise mechanisms responsible for biological motion perception are still to be elucidated, but multiple studies suggest that both bottom-up and top-down processing is required (reviewed in Blake & Shiffrar, 2007). The lower level spatiotemporal integration may be similar to other cases of structure-from-motion, but at later stages this will be guided by top-down constraints (priors) of biological body and movement structure. Evidence for such an interplay between these processes comes from a study by Neri (2009) who suggests that bottom-up local moving elements are first grouped to form separate limbs, which are subsequently integrated to the whole Gestalt of a moving agent. Two other indications that top-down influences are important in biological motion perception can be found in the literature. Firstly, psychophysical studies find that local motion perception is affected by being embedded in a global (biological) form (Chatterjee, Freyd, & Shiffrar, 1996; Tadin, Lappin, Blake, & Grossman, 2002). A second indication comes from a set of neuroimaging studies reporting that activity in brain regions sensitive to motion (MT/V5) is also modulated by a global biological form (e.g., Kourtzi & Kanwisher, 2000; Peuskens, Vanrie, Verfaillie, & Orban, 2005).

In the light of these findings, inversion may not matter under CFS, because the second, higher-level and possibly top-down driven phase is precluded by CFS. Indeed, neuroimaging studies have mostly indicated that neural activity related to the suppressed stimulus is mainly restricted to early visual areas (Hesselmann & Malach, 2011; Sterzer, Stein, Ludwig, Rothkirch, & Hesselmann, 2014). Related to this, the current results are consistent with a recent series of behavioral studies that have called into question whether

high-level processing of a perceptually suppressed stimulus is at all possible (Hedger et al., 2015a; Hesselmann & Knops, 2014; Hesselmann & Moors, 2015; Heyman & Moors, 2014; P. Moors, Huygelier, et al., 2015; P. Moors, Wagemans, van Ee, & de-Wit, 2015). On a speculative note, this could imply that the inversion effects previously observed for faces and bodies might be driven more by a lower-level bottom up mechanism rather than genuine face- and/or body-specific processing mechanisms.

While suppression may interfere with the second, higher-level top-down type of processing underlying the perception of biological motion, we did find evidence that the first component, which is not specific to biological motion, is still possible under CFS. Specifically, scrambled figures that lack spatial and temporal coherence are slower to break through than intact figures (upright or inverted). Hence, at least for local ranges, the spatiotemporal coherence of invisible stimuli is processed. This idea is supported by recent studies of Kaunitz et al. (2013) and Chung and Khuu (2014), who have found that coherently moving dots are better detected under CFS than randomly moving dots. Thus, it seems to be the case that, during CFS, a limited form of spatiotemporal integration takes place yielding facilitated access to awareness for spatiotemporally coherent stimuli. However, this does not seem involve higher-level, meaningful Gestalts such as point-light figures. Following up on this, two points deserve some discussion. First, it is interesting to note that Kaunitz et al. (2013) also included a biological motion stimulus in their experiments and failed to observe a *scrambling* effect. However, their implementation of scrambling the walkers also involved rotating the individual elements, which might imply that their scrambling resembles our inversion manipulation more than our scrambling manipulation. Furthermore, our sample size was about twice as big as in the study of Kaunitz et al. (2013), increasing our statistical power to detect an effect of motion coherence in biological motion stimuli. Second, a recent study by Faivre and Koch (2014) reported evidence that observers could adapt to the gender of invisible point-light walkers, which seems to suggest that the point-light walkers were fully integrated during CFS. However, the results also showed that kinematic rather than structural cues mainly contributed to the adaptation effect. Therefore, these results could also be interpreted in the sense that observers, during CFS, adapt to local parts of the motion stimulus, rather than adapting to the global, structured, whole stimulus.

One could ask whether the absence of an inversion effect has any implications for the often assumed selective "exquisite sensitivity" of humans to biological motion stimuli (Pavlova, 2012). Here, it should be noted that this very idea has been questioned before in other contexts. For example, when visibility of point-light figures is reduced by a noise mask (instead of suppressed as in CFS), Hiris (2007) demonstrated that people can detect biological motion just as well as non-biological motion, where the latter consisted of translating or rotating dot structures without biological form (e.g., rectangles). Related to this, Jastorff, Kourtzi, and Giese (2006) showed that complex movement patterns that are not based on biological, familiar shapes, can be learned just as quickly as biological motion stimuli, and the former also resulted in a strong orientation dependence (inversion effect) in discrimination performance. The authors suggest that top-down guidance in perception may emerge quickly as long as there is an articulated "skeleton" underlying complex movements. Together these studies suggest that the detection nor the inversion effect of biological motion stimuli is special. However, this does not do away with the inferential richness of these stimuli, i.e the fact that we readily attribute gender, intentions, emotions, etc. to these figures, once detected (which may sometimes be confused with detection sensitivity per se). However, it does urge us to devise better control stimuli to examine the contributions of bottom-up and top-down processing, of local integration and (learned) priors concerning global form. In this respect, although little is known about the precise mechanisms of CFS, we would argue that the dissociation we report here shows that it is fruitful to apply CFS to complex, multi-level, but well-controlled stimuli such as point-light figures to probe different stages or types of processing.

CONCLUSION

To summarize, we found that spatiotemporal coherence of point-light figures can facilitate access to awareness during CFS, because when point-lights are spatially scrambled and out of phase with each other, they take longer to break through. Consistent with previous studies, this means that there are limits to configural processing under CFS, even for what are usually described as biologically relevant stimuli. Contrary to what is consistently found for faces, our biological motion stimuli did not show an inversion effect under suppression. This implies that the characteristic biological motion processing is

hindered under suppression, possibly because it requires higher and/or top-down processing that is prevented by CFS.

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Chapter 8.
Faces in commonly experienced configurations enter awareness faster due to their curvature relative to fixation.

The extent to which perceptually suppressed face stimuli are still processed has been extensively studied using the continuous flash suppression paradigm (CFS). Studies that rely on breaking CFS (b-CFS), in which the time it takes for an initially suppressed stimulus to become detectable is measured, have provided evidence for relatively complex processing of invisible face stimuli. In contrast, adaptation and neuroimaging studies have shown that perceptually suppressed faces are only processed for a limited set of features, such as its general shape. In this study, we asked whether perceptually suppressed face stimuli presented in their commonly experienced configuration would break suppression faster than when presented in an uncommonly experienced configuration. This study was motivated by a recent neuroimaging study showing that commonly experienced face configurations are more strongly represented in the fusiform face area. Our findings revealed that faces presented in commonly experienced configurations indeed broke suppression faster, yet this effect did not interact with face inversion suggesting that, in a b-CFS context, perceptually suppressed faces are potentially not processed by specialized (high-level) face processing mechanisms. Rather, our pattern of results is consistent with an interpretation based on the processing of more basic visual properties such as convexity.

Moors, P., Wagemans, J., de-Wit, L. (2016). Faces in commonly experienced configurations enter awareness faster due to their curvature relative to fixation. *PeerJ*, 4:e1565.

INTRODUCTION

The extent to which invisible stimuli are still processed has become a popular line of research over the last decades (Dehaene & Changeux, 2011; Hesselmann & Moors, 2015). One particularly compelling paradigm to render visual stimuli invisible is continuous flash suppression (CFS) (Tsuchiya & Koch, 2005). In CFS, a salient dynamic pattern composed of various colored shapes is presented to one eye while another stimulus is presented to the other eye. Due to the dynamic nature of the mask, the other stimulus is perceptually suppressed and invisible to observers for a time period on the order of seconds. CFS has been implemented in various ways to study processing of perceptually suppressed stimuli, one being the breaking CFS paradigm (b-CFS) (Gayet et al., 2014; Stein, Hebart, et al., 2011). Here, the contrast of the initially suppressed stimulus is gradually increased until it causes a perceptual breakthrough (i.e., becomes detectable to the observer). The breakthrough or suppression time is then used as an index of the strength of the representation of that visual stimulus during suppression. That is, as in regular binocular rivalry, 'stimulus strength' is predicted to influence suppression durations such that stronger stimulus representations break CFS faster than weaker stimuli (Jiang et al., 2007; Stein, Hebart, et al., 2011). Here, it should be noted however that 'stimulus strength' is not a well-defined construct and that there is some debate as to which factors contribute exactly to differences in suppression times. That is, the factors driving breakthroughs could be low-level or high-level (Gayet, Van Der Stigchel & Paffen, 2014; Hesselmann & Moors, 2015), or involve the feature overlap between the CFS mask and the suppressed stimulus (P. Moors et al., 2014; E. Yang & Blake, 2012).

A number of studies have considered the degree to which face stimuli are still processed while perceptually suppressed and have used the b-CFS paradigm, amongst others, to tackle this question (for a review of unconscious face processing, not limited to CFS studies only, see Axelrod, Bar & Rees, 2015). A now-classic study by Jiang, Costello and He (2007) showed that upright face stimuli broke suppression faster than inverted face stimuli, resembling the well-known face inversion effect for consciously presented stimuli (Farah et al., 1995; Yin, 1969). Following this study, several b-CFS studies have replicated this face inversion effect (Gobbini, Gors, Halchenko, Rogers, et al., 2013; Gobbini, Gors, Halchenko, Hughes, & Cipolli, 2013; Heyman & Moors, 2014; Stein, End, & Sterzer, 2014; Stein, Hebart, et

al., 2011; Stein, Peelen, et al., 2011; Stein & Sterzer, 2012; Stein, Sterzer, et al., 2012; G. Zhou, Zhang, Liu, Yang, & Qu, 2010). Other studies have furthermore indicated that stimulus-related factors such as eye gaze (Chen & Yeh, 2012; Gobbini, Gors, Halchenko, Hughes, et al., 2013; Stein, Senju, Peelen, & Sterzer, 2011; Xu et al., 2011), facial expression (Capitão et al., 2014; Stein & Sterzer, 2012; Sterzer, Hilgenfeldt, Freudenberg, Bermpohl, & Adli, 2011; E. Yang et al., 2007), face identity (Geng, Zhang, Li, Tao, & Xu, 2012; Gobbini, Gors, Halchenko, Rogers, et al., 2013), face race (Stein, End, et al., 2014), or the trustworthiness or dominance of a face (Stewart et al., 2012) can influence suppression times. Taken together, these findings seem to suggest that, while perceptually suppressed, the representation of a face stimulus is a fairly integrated one involving the high-level analysis of several complex features.

In apparent contrast with these b-CFS findings, a more complicated pattern of results has arisen from studies that rely on adaptation to invisible face stimuli or investigate the representation of invisible face stimuli using neuroimaging techniques. For example, adaptation studies have indicated that visual awareness of a face is required for adaptation to complex features such as facial expression (E. Yang, Hong, et al., 2010), face race or gender (Amihai, Deouell & Bentin, 2011), face identity (Moradi et al., 2005), face shape (Stein & Sterzer, 2011), or eye gaze (Stein, Peelen, et al., 2012). The main conclusion of these studies is that adaptation effects for invisible stimuli are sometimes observed, but they are largely specific to the adapted eye and size of the stimulus. For example, Stein and Sterzer (2011) observed face shape aftereffects for fully invisible stimuli, yet these aftereffects were only observed if the test stimulus had the same size as the adaptor and was also presented to the same eye as the adaptor. This suggests that the adaptation occurred at a low level of processing, and was specific to simple features such as its exact size and shape. Similarly, neuroimaging studies have shown that neural responses to invisible face stimuli are strongly reduced in the fusiform face area (Jiang & He, 2006; Sterzer et al., 2014), although the pattern of activation still enables the successful decoding of certain stimulus distinctions (Sterzer, Haynes & Rees, 2008; Sterzer, Jalkanen & Rees, 2009).

Taken together, behavioral studies relying on adaptation and neuroimaging studies call into question whether the results obtained using the b-CFS paradigm are genuinely attributable to high-level configural processing of the invisible face. Rather, they suggest that the representation of the perceptually suppressed face is limited to lower-level aspects such

as its general shape. Therefore, in this study, we were interested to further study the representation of a perceptually suppressed face in a b-CFS context, capitalizing on the findings of a recent neuroimaging study. That is, Chan et al. (2010) recently showed that representations of body parts and faces were strongest in the extrastriate body area and fusiform face area, respectively, when they were presented in their commonly experienced configuration (e.g., the left side of a face presented in the right visual field). This result is intriguing since all conditions simply involved presenting the same stimulus (e.g., right or left side of a face) to a different side of the visual field. Thus, if stimulus strength influences suppression time, we would predict that perceptually suppressed face stimuli presented in their commonly experienced configuration would break suppression faster compared to those presented in the other part of the visual field. Moreover, given that the effect for the face stimuli seems to be specific to the fusiform face area, the presence of such an effect in a b-CFS setup could be indicative of the extent to which invisible face stimuli are processed during suppression. To this end, we also included a face inversion condition. That is, if a congruency effect is observed, this inversion condition will enable us to test whether this effect is dependent on specialized processing for upright faces.

METHODS

Participants

43 people participated in the experiment. All participants had normal or corrected-to-normal vision and were naïve with respect to the purposes of the study. The study was approved by the local ethics committee of the faculty (the Social and Societal Ethics Committee of the KU Leuven (SMEC) under the approval number G-2014 08 033). All participants provided written informed consent before the start of the experiment.

Apparatus

Stimuli were shown on two 19.8-in. Sony Trinitron GDM F500-R (2048 x 1536 pixels at 60 Hz, for each) monitors driven by a DELL Precision T3400 computer with an Intel Core Quad CPU Q9300 2.5 GHz processor running on Windows XP. Binocular presentation was achieved by a custom made stereo set-up. Two CRT monitors, which stood opposite to each other (distance of 220 cm), projected to the left and right eye respectively via two mirrors

placed at a distance of 110 cm from the screen. A head- and chin rest (15 cm from the mirrors) was used to stabilize fixation. The effective viewing distance was 125 cm. Stimulus presentation, timing and keyboard responses were controlled with custom software programmed in Python using the PsychoPy library (Peirce, 2007, 2009).

Stimuli

The background of the display consisted of a random checkerboard pattern to achieve stable binocular fusion. The size of the individual elements of the checkerboard was equal to 0.34°. In both eyes, a black frame (10° by 10°) was superimposed on the checkerboard pattern, onto which the stimuli would be presented. A black (eye dominance measurement) or white (main experiment) fixation cross was continuously present during the experiment (size 0.5 by 0.5°). In the eye dominance measurement phase, the target consisted of an arrow (maximal width 4°, maximal height 2°) and the CFS mask consisted of 150 squares with randomly picked sizes between 1 and 2° and a random luminance value (range: 1 – 100 cd/m^2).

We obtained the stimuli used in the original study of Chan et al. (2010) and used a subset of those in this study (see Figure 8.1A). That is, we only used the face configurations of their stimulus set, which consisted of four different half-face exemplars (size 3° of visual angle). For the specific details of the stimulus generation procedure, we refer to the original study. In the main experiment, the CFS mask (6° x 6°) consisted of 200 grayscale squares with a random size between 0.75° and 1.5°. In all parts of the experiments, the CFS mask refreshed its contents every 100 ms (i.e., at 10Hz).

Procedure

In the first part of the experiment, observers performed an eye dominance task according to the procedure outlined by Yang, Blake and McDonald (2010). That is, on each trial, the CFS mask was presented to one of the observer's eyes and an arrow stimulus to the other eye. The arrow stimulus gradually increased from 0% to 100% contrast over a period of 2 seconds after which it remained present at full contrast. Upon breakthrough of the arrow stimulus, participants had to indicate as quickly as possible whether the arrow was pointing to the left or right. Participants performed this task for 80 trials in total (40 trials per eye). The

dominant eye was determined by taking the eye for which the mean suppression was the lowest. In all subsequent phases of the experiment, the CFS mask was always presented to the dominant eye.

In the main part of the experiment each trial consisted of a 1 second fixation phase after which the CFS mask was presented to the dominant eye and the face stimulus to the non-dominant eye (Figure 8.1B). The face stimulus gradually increased from 0% to 100% contrast in a period of 1 second after which it remained on screen at full contrast until the participants' response. Upon breakthrough, participants had to indicate as quickly as possible whether the face stimulus was presented to the left or right of fixation by means of a button press. Prior to the start of the main experiment, participants first completed a practice block to become acquainted with the task.

Design

The experiment consisted of a 2 x 2 x 2 full-factorial within-subjects design. Each stimulus (left or right side of a face) was presented in the left or right visual field in an upright or inverted fashion. Participants completed a total of 96 trials. The practice block consisted of 8 trials.

Data analysis

All analyses were performed in R, a statistical programming language (R Core Team, 2014). All statistical analyses were performed in a Bayesian framework, relying on model selection through Bayes Factors (Rouder et al., 2009, 2012). In Bayesian statistics, statistical inference is performed by relying on Bayes' rule:

$$p(\theta|D) = \frac{p(D|\theta)p(\theta)}{p(D)}$$

where θ refers to a vector of parameters (e.g., the effect parameters of an ANOVA model) and D to the data under consideration. In Bayes' rule, the prior probability distribution, $p(\theta)$, is then updated by the likelihood $p(D|\theta)$ to yield the posterior probability distribution, $p(\theta|D)$. In the Bayes Factor approach, the focus is on the marginal likelihood, $p(D)$:

$$p(D) = \int p(D|\theta)p(\theta)d\theta$$

The Bayes factor then refers to the ratio of marginal likelihoods of different statistical models under consideration (e.g., a model with main effects of congruency and inversion versus a model with only a main effect of congruency), quantifying the change from prior to posterior model odds:

$$\frac{p(M_1|D)}{p(M_2|D)} = \frac{p(M_1)}{p(M_2)} \frac{p(D|M_1)}{p(D|M_2)}$$

where

$$BF_{12} = \frac{p(D|M_1)}{p(D|M_2)} = \frac{\int_{\Theta_1} p(D|\theta)p(\theta)d\theta}{\int_{\Theta_2} p(D|\theta)p(\theta)d\theta}$$

In itself, the Bayes Factor can be interpreted as a *relative* measure of evidence for one statistical model compared to another (e.g., a model with two main effects versus a model with two main effects and their interaction). That is, the value of the Bayes Factor has no absolute meaning, and should always be interpreted relative to the statistical models under consideration.

All Bayes Factors were computed using the R package BayesFactor version 0.9.11-1 (Morey & Rouder, 2015) using all default settings. The statistical models for which Bayes Factors were computed are akin to classical repeated measures ANOVA models, yet including random intercepts for both subject as well as stimulus (given that we used different face exemplars in our experiment, also known as a crossed random effects model: see Clark, 1973; Baayen, Davidson & Bates, 2008). Rouder et al. (2012) developed a default class of Bayes Factors for ANOVA designs and described the prior distributions used for calculating these Bayes Factors in detail. In short, normal distributions are used as priors for the fixed and random effects. These have a prior mean of zero, and an independent variance (width) for each of these effects, based on so-called g-priors developed by Zellner and Siow (1980). The settings that can be adjusted in the BayesFactor package relate to the width of the prior distributions on the fixed and random effects (quantified by the scaling factor *r*). For the fixed effects we used the "wide" setting ($r = 0.5$) whereas for the random effects the "nuisance" option was used ($r = 1$). Following the classification proposed by Jeffreys (1961),

Bayes Factors > 3 are considered to be convincing evidence for one model compared to another. In this paper, all Bayes Factors quantify how much more likely the best fitting model is compared to another model. That is, the best fitting model is always put in the numerator, whereas the other models under consideration are put in the denominator of the Bayes Factor equation.

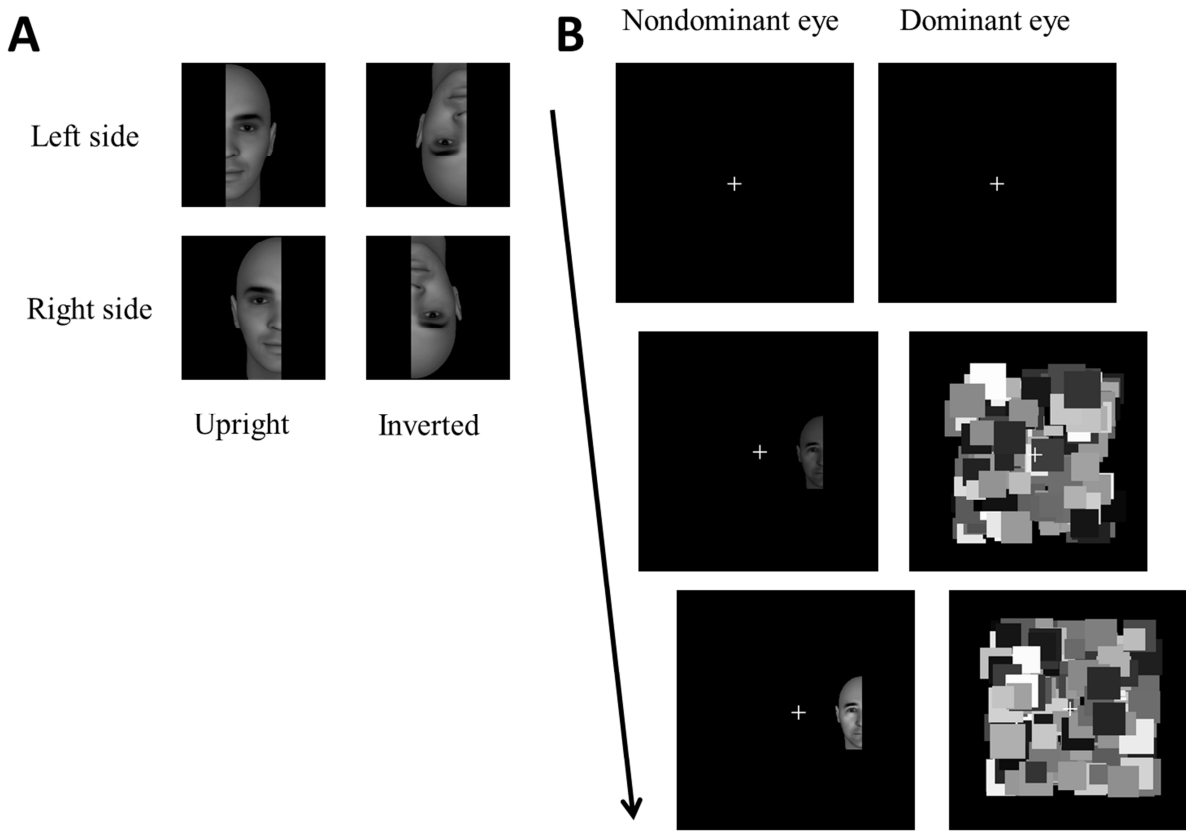

Figure 8.1. Stimuli and procedure. (A) Four different configurations for one face exemplar. Each configuration was presented either to the left or right side of the fixation cross. Presenting the top left stimulus to the right side of fixation would constitute an upright, congruent stimulus. (B) Trial sequence used in the experiment. Each trial started with a fixation period of 1 second after which the face stimulus was presented to the non-dominant eye and the CFS mask to the dominant eye. The face stimulus gradually increased in contrast and remained present at 100% contrast until the participants' response.

RESULTS

Before subjecting the data to any analysis, suppression times were first log transformed to account for their positive skew. Only correct responses were considered. Outliers were defined as suppression times that deviated more than three standard deviations from the mean suppression time (for each observer separately) and these were

also excluded from the analysis. This led to a removal of 5.5% of the data. To facilitate the interpretation of the data, we converted the factors visual field and stimulus side to a single variable termed 'congruency'. A congruent stimulus would be one that constitutes a commonly experienced configuration (e.g., right side of the face in the left visual field, assuming fixation in the center). For inverted stimuli, we applied the same transformation such that congruent stimuli would be the ones for which the overall configuration would be the same (e.g., an inverted left side of the face would now have to be presented in the left side of the visual field to be coded as congruent). Table 8.1 depicts the results of the Bayes Factor analysis. The betting fitting model (BF = 1) is one that includes a main effect of congruency and a main effect of inversion. This model is preferred 5.2 times over a model including also the interaction between the main effects. Furthermore, a model including only a main effect of inversion and no congruency effect is 3.6 times less likely than the best fitting model. For all other models (e.g., a model with a main effect of congruency only), the best fitting model was more than 100 more likely (i.e., BFs > 100). The mean suppression times for all combinations of congruency and face inversion are depicted in Figure 8.2. In line with the Bayes Factor analysis, inverted faces yielded longer suppression times than upright faces (the well-known face inversion effect). Furthermore, face stimuli presented in congruent configurations broke suppression faster than the incongruent ones, yet this main effect did was not modulated by stimulus inversion.

Table 8.1. Bayes Factor analysis.

Model	**Bayes Factor**
Congruency + Inversion	1
Inversion	3.6
Congruency * Inversion	5.2
All other models	> 100

Note. All Bayes Factors can be interpreted relative to the best fitting model (for which the Bayes Factor equals 1). A + indicates that only main effects are included in the model. A * denotes both main effects and the interaction between the conditions.

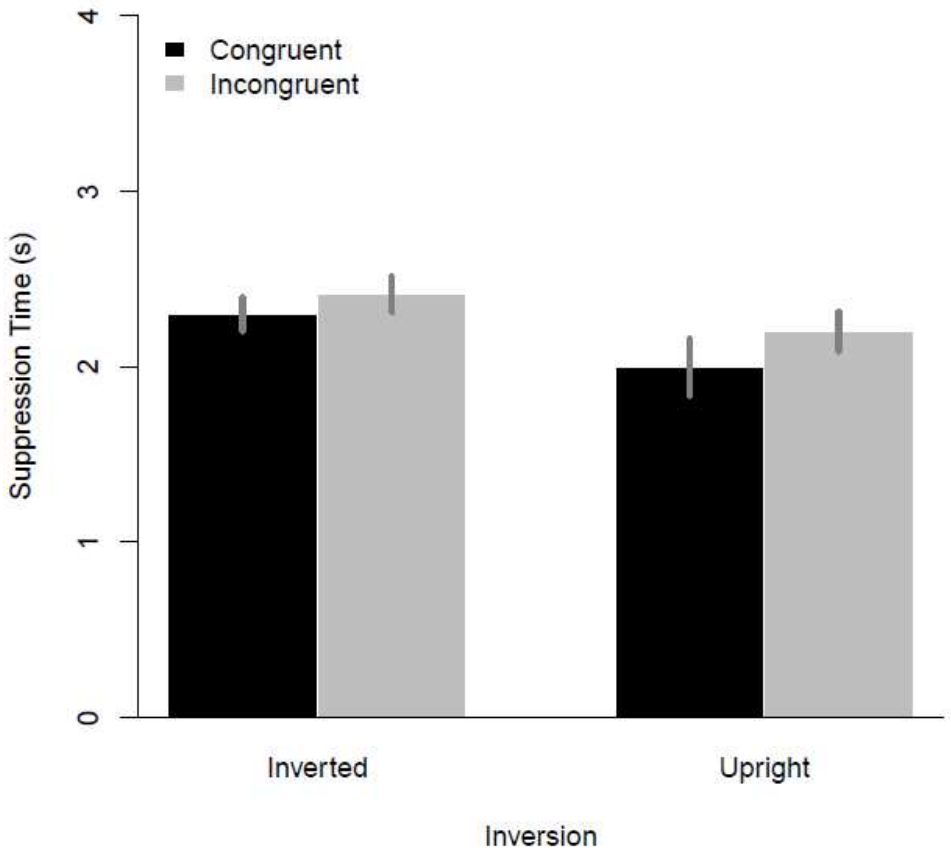

Figure 8.2. Mean suppression times for all conditions. Error bars denote 95% within-subject confidence intervals as described by (Morey, 2008).

DISCUSSION AND CONCLUSION

The goal of this study was to assess whether face stimuli presented in their commonly experienced configurations would break suppression faster than the same stimuli presented in other configurations. Our results indicated that this indeed was the case, yet the effect was not specific for upright face stimuli. That is, similar configurations also broke suppression faster when they were presented inverted rather than upright. This result implies that shape differences relative to fixation were responsible for the observed congruency effect rather than processing mechanisms specific for upright faces.

This study was motivated by the fact that a lot of b-CFS studies on face processing obtained evidence for relatively complex (high-level) processing of invisible faces during CFS. In contrast, studies relying on adaptation or neuroimaging techniques consistently showed that processing of invisible faces is severely reduced compared to visible faces and is possibly only specific to the general face shape rather than the identity, facial expression, or

other high-level face attributes. Therefore, we decided to capitalize on the findings of a neuroimaging study in which it was shown that the pattern of responses in the fusiform face area was strongest for face stimuli presented in their commonly experienced (congruent) configuration. Assuming that stimuli with a strong representation break suppression faster, one would predict the same difference between congruent and incongruent configurations to be observed in a b-CFS setup. Moreover, given the specificity of the effect to the fusiform face area, we also predicted that the effect should be absent or at least greatly reduced for inverted faces (Yovel & Kanwisher, 2005). As highlighted above, our results indicated both an effect of configuration as well as inversion but no interaction between those factors. This indicates that the differences in suppression time between conditions are more likely attributable to shape-specific differences between conditions rather than mechanisms relying on the configural processing of faces, which are known to be affected by inversion (Yin, 1969; Farah, Tanaka, & Drain, 1995). Indeed, studies on holistic face perception have shown that face inversion is a stimulus manipulation that strongly influences performance on a wide range of tasks (for a review, see Rossion, 2008; Van Belle et al., 2010). Nevertheless, it has also been argued that inverted faces can still be processed holistically (Richler, Mack, Palmeri, & Gauthier, 2011). This last study has mainly indicated qualitatively similar patterns for upright and inverted faces, but still observed quantitative differences. In our study however, the congruency effect was also quantitatively similar between upright and inverted faces given the absence of an interaction between face inversion and congruency. Therefore, we think the most parsimonious explanation of our results is one that does not rely on face-specific (high-level) configural processing of perceptually suppressed face stimuli.

One particularly important difference between the stimuli presented in both types of configurations is the curvature of the face shape relative to fixation. That is, in congruent configurations, the curved contour is convex relative to fixation compared to being concave in the incongruent configurations. Several behavioral studies have shown that convex features are often perceptually dominant, for instance, in determining figure-ground relationships or shape similarity (Bertamini & Wagemans, 2013; Kanizsa & Gerbino, 1976). Moreover, neurophysiological recordings have shown a similar bias towards convex features in macaque area V4 (Pasupathy & Connor, 1999). Last, a recent fMRI study has shown that cortical area LOC shows higher sensitivity for convex rather than concave shapes

(Haushofer, Baker, Livingstone, & Kanwisher, 2008). Although our study only consisted of face stimuli, the pattern of results observed in this study is similar to what would be predicted based on a convexity/concavity account. Thus, in the light of these studies, we can speculate that our findings can be interpreted as potentially reflecting the heightened sensitivity of the visual system to convex features (relative to fixation).

This interpretation is in accord with a larger set of studies that has questioned evidence of high-level processing of stimuli suppressed through CFS. For example, Hedger, Adams and Garner (2015a) recently showed that the advantage of fearful faces breaking suppression faster than neutral ones is predicted by effective contrast of the stimuli. Furthermore, another recent study by the same group observed that attentional orienting due to threat stimuli is completely absent when threatening stimuli were rendered completely invisible (Hedger et al., 2015a). Other studies have cast doubt on whether invisible words can be processed (Heyman & Moors, 2014), numerosity can be extracted during suppression (Hesselmann, Darcy, Sterzer, & Knops, 2015; Hesselmann & Knops, 2014; Liu, Zhang, Zhao, Liu, & Li, 2013), or integration between a suppressed visual looming stimulus and a supraliminal auditory stimulus can occur (P. Moors, Huygelier, et al., 2015).

In sum, the results of this study provide evidence that stimuli that are more strongly represented in the visual cortex break suppression faster than other stimuli. However, the fact that the observed congruency effect was not specific for upright face stimuli indicates that the face stimuli used in this study were presumably not processed by specialized face recognition mechanisms, but rather at a more basic level limited to more general shape properties such as convexity.

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Chapter 9.
Scene integration without awareness: No conclusive evidence for processing scene congruency during continuous flash suppression

A recent study showed that scenes with a semantically incongruent object-background relationship break interocular suppression faster than scenes with a semantically congruent relationship. These results implied that semantic relations between objects and background of a scene could be extracted in the absence of visual awareness of the stimulus. In this study, we assessed the replicability of this finding and tried to rule out an alternative explanation based on low-level differences between the stimuli. Furthermore, we used a Bayesian analysis to quantify the evidence in favor of the presence or absence of a scene congruency effect. Across three experiments, we found no convincing evidence for a scene congruency effect nor a modulation of scene congruency by scene inversion. These findings question the generalizability of previous observations and cast doubt on whether genuine semantic processing of object-background relationships in scenes can manifest during interocular suppression.

Moors, P., Boelens, D., van Overwalle, J., & Wagemans, J. (2016). Scene integration without awareness: No conclusive evidence for processing scene congruency during continuous flash suppression. Psychological Science, in press.

INTRODUCTION

Understanding the scope and limits of unconscious visual processing has become a central research topic in cognitive neuroscience (Dehaene & Changeux, 2011). A recent study claimed to have obtained evidence that complex, high-level visual scene processing can happen unconsciously (Mudrik, Breska, Lamy, & Deouell, 2011). These authors presented participants with scenes that were rendered invisible through continuous flash suppression (CFS, Tsuchiya & Koch, 2005). Scene congruency was manipulated and participants had to indicate when an initially suppressed scene broke suppression. Mudrik et al. (2011) observed that incongruent scenes broke suppression faster than congruent scenes. This led the authors to argue that consciousness of a scene is not required for high-level scene processing mechanisms to unfold and hence extract the congruency relation between object and background.

Because the results of Mudrik et al. (2011) have profound implications for theories on the extent of unconscious visual processing during CFS, it is of utmost importance that the congruency effect can be attributed to genuine scene processing mechanisms rather than differences between scenes on low-level visual aspects. Although image analyses (Itti & Koch, 2000; Neumann & Gegenfurtner, 2006) on the stimulus set used in these experiments did not seem to reveal any consistent bias on low-level visual aspects for the (in)congruent category, a stronger control for image-related characteristics is to include an experimental condition in which the scenes are inverted, which dramatically reduces their identifiability while fully preserving the low-level image properties.

The goal of this study was threefold. First, we wanted to assess the replicability of the original findings. Second, by including a scene inversion condition, we wanted to rule out any potential low-level confounds related to the particular stimulus set. Third, we complement the traditional repeated measures ANOVA with a Bayesian analysis based on linear mixed-effects modeling with crossed random effects for participants and stimuli (Clark, 1973). Given that the experiment consists of presenting various exemplars of congruent and incongruent scenes to participants, a random effect of stimulus should also be included if one aims to generalize to the population of congruent and incongruent scenes. Furthermore, Bayesian statistics allows to quantify the evidence for the *absence* of an effect (Rouder, Speckman, Sun, Morey, & Iverson, 2009).

EXPERIMENT 1

Experiment 1 consisted of a replication experiment of Mudrik et al. (2011), using the same stimuli and methods, yet also including a scene inversion condition. If the results observed in Mudrik et al. (2011) are genuinely attributable to unconscious scene processing, we predicted to observe a scene congruency effect in the upright, but not in the inverted condition. Conversely, if the effect does not pertain to processing the semantic aspects of the interocularly suppressed scenes, we predict a similar congruency effect in the inverted condition and, critically, no (statistical) interaction between scene inversion and scene congruency.

METHODS

Participants

45 people participated in the study in return for money or course credit. All participants were naïve with respect to the purposes of the study and had normal or corrected-to-normal vision. Because the original sample size of 18 participants used in Mudrik et al. (2011) yielded a post-hoc power of 75%, we decided to substantially increase the sample such that the power based on the effect size reported in the original study was 99% for this experiment. This increase in sample size was further motivated by the fact that one needs sufficient measurements for each item to fit a linear mixed-effects model with crossed random effects.

Apparatus

Stimuli were shown on two 19.8-in. Sony Trinitron GDM F500-R (2048 x 1536 pixels at 60 Hz, for each) monitors driven by a DELL Precision T3400 computer with an Intel Core Quad CPU Q9300 2.5 GHz processor running on Windows XP. Binocular presentation was achieved by a custom made stereo set-up. Two CRT monitors, which stood opposite to each other (distance of 220 cm), projected to the left and right eye respectively via two mirrors placed at a distance of 110 cm from the screen. A head- and chin rest (15 cm from the mirrors) was used to stabilize fixation. The effective viewing distance was 125 cm. Stimulus presentation, timing and keyboard responses were controlled with custom software programmed in Python using the PsychoPy library (Peirce, 2007, 2009).

Stimuli

The background of the display consisted of a random checkerboard pattern to achieve stable binocular fusion. The size of the individual elements of the checkerboard was equal to 0.34°. In both eyes, a gray frame (10° by 10°) was superimposed on the checkerboard pattern to present the stimuli. A black (eye dominance measurement) or white (main experiment) fixation cross was continuously present during the experiment (size 0.5 by 0.5°). In the eye dominance measurement phase, the target consisted of an arrow (maximal width 4°, maximal height 2°) and the CFS mask consisted of 150 gray squares with randomly picked sizes between 1 and 2° and a random luminance value for each element.

The scene stimuli (2.86° x 2.03°) were the same as in Mudrik et al. (2011). For a detailed description of the stimulus set, we refer to the original study. In short, the scenes depicted various human actions involving a certain object. Both congruent and incongruent versions of the scene were created by pasting an object into the scene that was related or unrelated to the action (Mudrik et al., 2011). In the main experiment, the CFS mask consisted of 200 square elements with a randomly chosen color and size between 0.75° and 1.5° for each element. The positions of the elements were generated in a 5.26° x 5.26° square window centered at fixation. Because the maximal size of each element was 1.5°, the effective size of the CFS mask was thus minimally 5.26° x 5.26° and maximally 6.76° x 6.76° (compared to always 5.26° x 5.26° in the original study). In both the eye dominance phase as well as the main experiment, the refresh rate of the CFS mask was set at 10 Hz.

Procedure

In the first part of the experiment, participants' eye dominance was measured using the method of Yang, Blake, and McDonald (2010). On every trial, a fixation cross was presented for 1 second. Next, an arrow that gradually increased to 100% contrast in 2 seconds was presented to one eye and the CFS mask in the other. Upon breakthrough of the arrow, participants had to indicate the direction of an arrow by pressing "1" or "3" on a numerical keyboard for left and right pointing directions, respectively. The CFS mask was randomly presented to the left or right eye (40 trials per eye) for a total of 80 trials. The dominant eye was determined as the eye in which mean suppression time was shortest when the arrow was presented to that eye. In all subsequent phases of the experiment, the CFS

mask should have been presented to the participants' dominant eye. Due to a programming error, however, the mask was always presented to the participant's right eye (in Experiments 1 and 2). In this experiment, 44% of the observers was left-, rather than right-eye dominant according to the criterion defined above , and thus received the mask in their non-dominant (right) eye.

The main experiment consisted of 160 CFS trials, divided into four blocks of 40 trials. In each block, all four conditions (all combinations of scene congruency and scene inversion) were balanced. Only one version of a scene was presented in each block. Within each block, the ordering of conditions was completely randomized, whereas in the original study a constraint was used that items of the same type could not be presented on four or more consecutive trials. Before the start of the main experiment, participants completed 16 practice trials based on four scenes that were not included in the main experiment.

On each trial, a CFS mask was presented to the participants' right eye while the scene was presented to the left eye and gradually increased from 0 to 100% contrast in steps of 10% every 100 ms. After the scene had reached full contrast, the CFS mask began to decrease in contrast to 0% over the course of 5.1 seconds. Upon breakthrough, participants had to indicate as fast as possible whether the scene was presented to the left or right of fixation (see Figure 9.1 for an overview of the trial sequence). Contrary to the original study, we did not perform a post-experimental rating session in which participants were asked to categorize all scene stimuli as being unusual or not. In the original study, incorrectly categorized stimuli were then removed prior to the start of the analysis. Because categorization performance could influence the results, we invited participants afterwards to perform the rating session in an online experiment. Here, we presented the scene stimuli in a random order and participants had to indicate for each stimulus whether it was unusual or not.

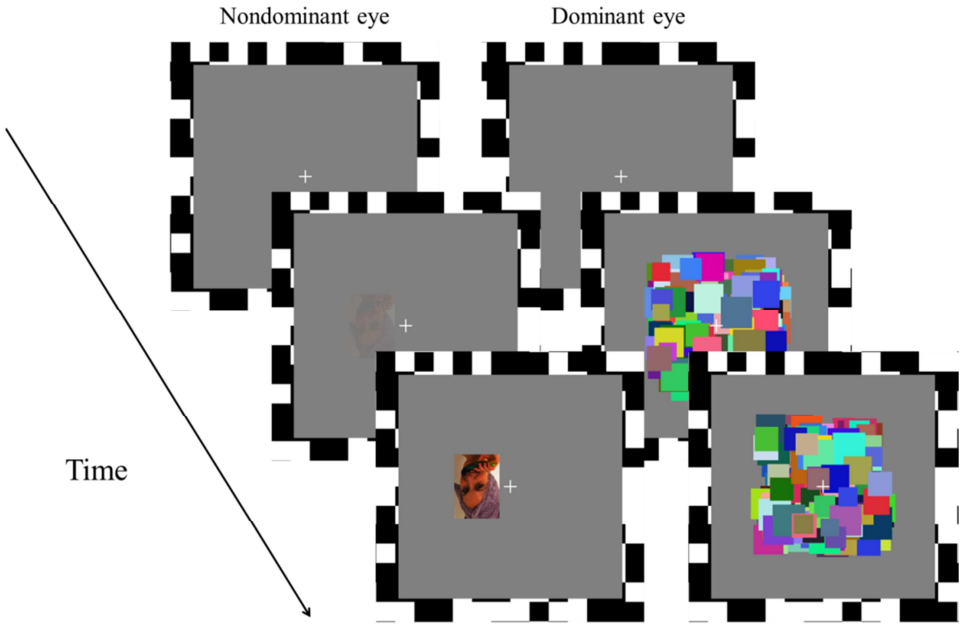

Figure 9.1. Trial sequence for all experiments. A fixation cross was presented to both eyes for 1 second, after which the scene stimulus was presented to the nondominant eye and the CFS mask to the dominant eye. The scene stimulus gradually increased in contrast for 1 second after which the CFS mask started decreasing in contrast for 5.1 seconds. Upon breakthrough, participants had to indicate as quickly as possible whether the scene stimulus was presented to the left or the right of the fixation cross.

RESULTS

All analyses were performed on the correct trials (M = 0.99, SD = 0.01) of which the suppression time did not exceed the time at which the mask reached 0% contrast (M = 0.92, SD = 0.13), after removing outliers defined as suppression times higher than the mean suppression time plus three times the standard deviation (for each observer separately, M = 0.005, SD = 0.007) . For the Bayesian analysis, we logarithmically transformed the suppression times to account for the positive skew in the suppression time distributions. Bayes Factors (BF) were computed to quantify the evidence for the presence/absence of a main effect or interaction. The R package BayesFactor (version 0.9.11-1, default settings, "medium" prior scale for fixed effects and "nuisance" prior scale for random effects) was used to compute the BFs (Morey & Rouder, 2015). All models were linear mixed-effects models with crossed random effects, including a random intercept for both participants and stimuli (Rouder, Morey, Speckman, & Province, 2012). To compute the BFs for the main

effects and interaction, we compared a full model (including the two main effects and interaction) with a reduced model in which the effect of interest was not included (i.e., similar to the classical repeated measures ANOVA). According to the classification provided by Jeffreys (1961) a BF of 3 constitutes substantial evidence for one model over the other whereas a BF of 10 is considered to be strong evidence for one model over the other. It is important to stress that Bayes Factors constitute a relative measure of evidence for one statistical model compared to another (i.e., it is a ratio of marginal likelihoods computed for two statistical models). In this paper, we always report Bayes Factors with the reduced model in the numerator and the full model in the denominator. Thus, BFs > 3 indicate evidence for the *absence* of a main effect or interaction under consideration. BFs < 0.3 indicate evidence for the *presence* of the effect under consideration. Because the Bayes Factor is asymmetric around 1, we visualize the Bayes Factors after logarithmically transforming them (with base 10 logarithm) such that a BF of 1 or -1 indicates strong evidence for the absence or presence of an effect, respectively.

Before analyzing the data, we first checked the consistency across observers with respect to which stimuli broke suppression fast and slow. That is, if the images were being processed during suppression, at least the low-level image characteristics should have an influence on suppression times and this should be apparent from calculating Cronbach's α for the suppression times. Indeed, Cronbach's α was high for the suppression durations (α= .83) indicating that there was consistency across observers in which stimuli broke suppression fast and slow.

A two-way repeated measures ANOVA on the mean correct suppression times revealed no main effect of scene congruency ($M_{congruent}$ = 2.53, $M_{incongruent}$ = 2.56; $SD_{congruent}$ = 0.87, $SD_{incongruent}$ = 0.85; $F(1,44) = 1.08$, $p = .30$, $d = -0.16$), a main effect of scene inversion ($M_{upright}$ = 2.49, $M_{inverted}$ = 2.61; $SD_{upright}$ = 0.87, $SD_{inverted}$ = 0.86; $F(1,44) = 16.1$, $p = .0002$, $d = 0.61$), and no interaction between scene congruency and scene inversion ($F(1,44) = 0.022$, $p = .88$, $d = -0.02$). As is apparent from Figure 9.2, inverted scenes on average broke suppression slower than upright scenes. Furthermore, congruent scenes on average broke suppression numerically faster than incongruent scenes, yet the p-value associated with this difference did not reach the significance threshold.

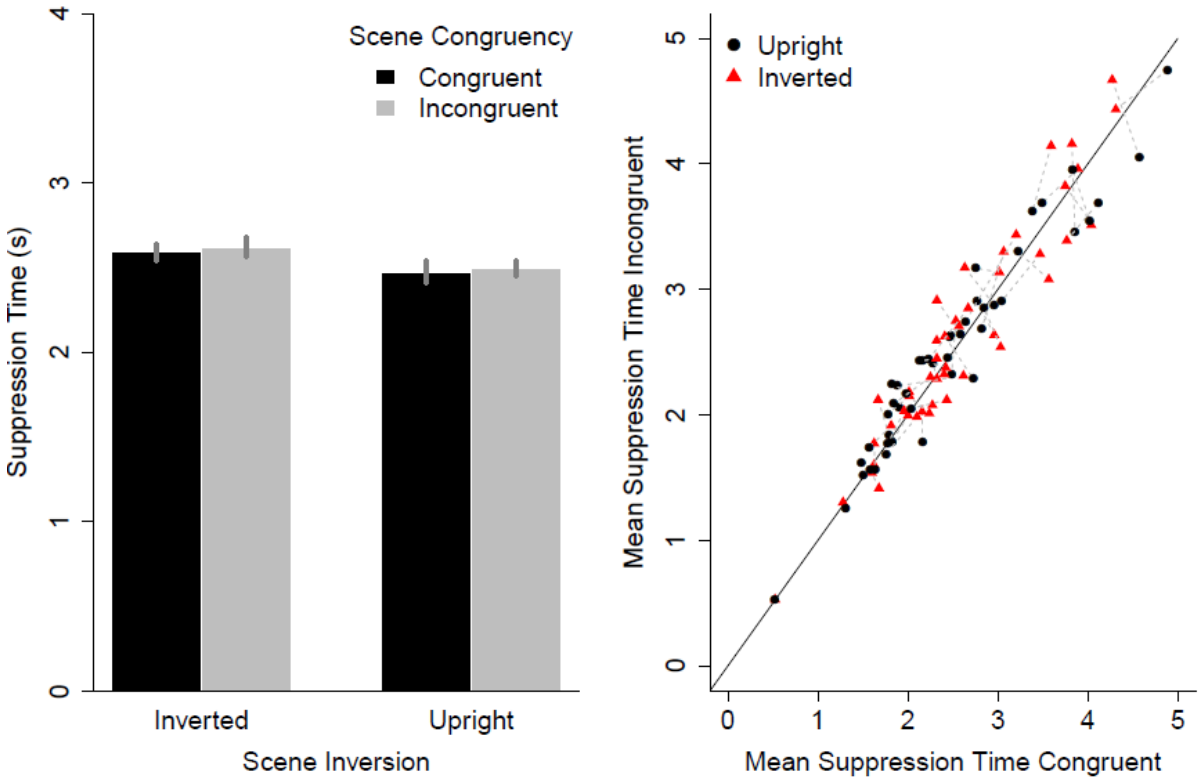

Figure 9.2. Results of Experiment 1. (left) Bar plot depicting mean suppression times for each condition. Error bars refer to 95% within-subject confidence intervals with the adjustment suggested by Morey (2008). (right) Scatter plot of mean suppression times for congruent and incongruent scenes for each participant. Black dots refer to the upright condition, red triangles to the inverted. The gray dashed lines connect the same participants.

The results of the repeated measures ANOVA were complemented by those of the Bayesian analysis. Specifically, the Bayes factor indicated strong evidence in favor of the absence of a scene congruency effect (BF = 17), strong evidence in favor of the presence of a scene inversion effect (BF < .01) and strong evidence for the absence of an interaction effect (BF = 24).

Figure 9.3 depicts a sequential analysis of the data for both statistical techniques. The upper panels depict how the p-values of the repeated measures ANOVA evolved as more participants were tested. As is apparent, several times throughout data collection, the main effect of congruency passed the significance threshold of .05. In contrast, the interaction never reached significance. In this respect, it is interesting to compare these patterns to the evolution of the Bayes Factors, which depict a different story. Here, the main effect of congruency nearly always hovers around a value of log10BF = 1 (i.e., a BF of 10), indicating

strong evidence for the absence of a congruency effect. The BF of the interaction effect also shows a gradual increase towards more evidence in favor of the absence of an interaction effect. Interestingly, for the main effect of inversion, both patterns show a gradual increase in evidence for an inversion effect.

Taking into account participants' categorization on the rating experiment did not change the results of this experiment (see Supplementary Materials). We also reanalyzed the data by excluding all participants for whom the CFS mask was presented to the non-dominant eye. Again, this did not influence the results (see Supplementary Materials).

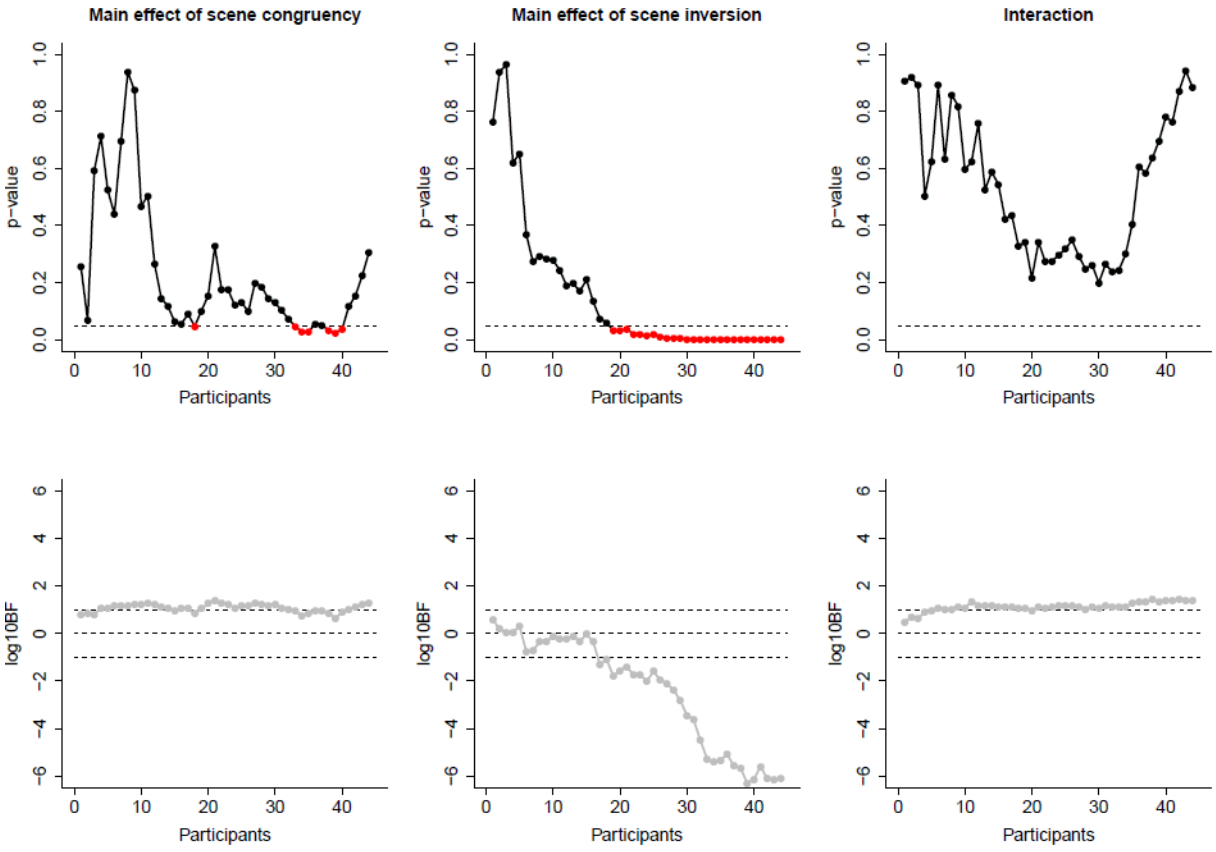

Figure 9.3. Sequential analysis of the data. (top row) Evolution of p-values as more data were collected for the main effects of scene congruency and scene inversion and their interaction, respectively. Black dots indicate p-values higher than .05 whereas red dots indicate p-values smaller than .05. (lower row) Evolution of Bayes Factors. Bayes Factors higher than 0 indicate evidence for the absence of an effect whereas Bayes Factors smaller than 0 indicate evidence for the presence of an effect.

DISCUSSION

The goal of Experiment 1 was to replicate the scene congruency effect observed in Mudrik, Breska, et al. (2011) and to assess whether it would be influenced by scene inversion. If genuine integration between the objects of a scene and its background can still proceed during CFS, one would predict that inverting the scenes would reduce or diminish the scene congruency effect. The results of Experiment 1 showed no convincing evidence for a main effect of scene congruency nor an interaction between scene congruency and scene inversion. In contrast, a reliable scene inversion effect was found in that upright scenes broke suppression faster than inverted scenes.

To increase our confidence in the absence of a scene congruency effect, we ran a second experiment in which we increased the number of trials for each observer. Furthermore, we dropped the inversion condition. This allowed us to run a quasi-exact replication of the original study and to assess whether increasing the number of trials for each observer would give us more power to detect a scene congruency effect. Furthermore, we decided to drop the mask fade-out procedure because this forced us to exclude a high number of trials for observers for which suppression was very effective.

EXPERIMENT 2

METHODS

Participants

24 new people participated in the study in return for money or course credit. All participants were naïve with respect to the purposes of the study and had normal or corrected-to-normal vision.

Apparatus and Stimuli

The set-up and stimuli were exactly the same as in Experiment 1. Inverted scenes were no longer included.

Procedure

The procedure was the same as in Experiment 1 except as noted here. In this experiment, 50% of participants was identified as left-eye dominant. The CFS mask was thus

presented to the non-dominant eye for half of the participants. We did no longer include the scene inversion condition. The experiment was set up such that each block constituted a fairly exact replication of the original study. That is, the experimental procedure was the same as in the original study and this procedure was repeated four times. We only did no longer include the mask fade-out procedure to ensure that the absence of a scene congruency effect could not be attributed by limiting the observations for observers for which suppression was very effective and thus excluding the upper tail of the suppression time distributions for these participants. Experiment 2 also did not include the post-experimental rating session, but we contacted the participants afterwards to complete it online, as we did for Experiment 1.

Design

Scene congruency was the only factor that was manipulated. Participants completed 8 practice trials, and the main experiment consisted of four repetitions of the full stimulus set amounting to 320 trials in total.

RESULTS

The results were analyzed in the same way as in Experiment 1. That is, all analyses were performed on the correct trials (first block: $M = 0.98$, $SD = 0.02$; all data: $M = 0.99$, $SD = 0.02$), after removing outliers defined as suppression times higher than the mean suppression time plus three times the standard deviation (for observers separately, first block: $M = 0.007$, $SD = 0.01$; all data: $M = 0.02$, $SD = 0.008$). We separately report analyses for the first block only (quasi-exact replication of the original study) and all four blocks combined.

First, we again analyzed the consistency of suppression times for images across observers. This again indicated high reliability for the suppression times of the images across observers ($\alpha = 0.67$ and 0.81, for the first block and all data respectively). Interestingly, the ordering of which items broke suppression fast and slow correlated highly across both experiments ($r = 0.89$, BF < 0.01).

As in Experiment 1, no effect of scene congruency was observed neither in the first block ($M_{congruent} = 2.87$, $M_{incongruent} = 2.90$; $SD_{congruent} = 1.09$, $SD_{incongruent} = 1.05$; $t(23) = -0.44$, $p = .66$, $d = -0.09$) nor when considering the data as a whole ($M_{congruent} = 2.34$, $M_{incongruent} = 2.36$; $SD_{congruent} =$

0.77, $SD_{incongruent}$ = 0.75; $t(23)$ = -0.69, p = .50, d = -0.14). Similarly, a BF analysis of the data always indicated convincing evidence for the absence of a scene congruency effect (BF = 15 and BF = 32, for the first block and all data, respectively).

As can be derived from the figure depicting the results of the sequential analysis, we crossed the significance boundary twice during the data collection process when only the first block would be considered. Increasing the number of trials per observer, however, never led to a significant main effect of congruency. Again, and in contrast to the results of the traditional analysis, the BF analysis showed a gradual increase in evidence for the absence of a scene congruency effect as the data came in.

Supplementary analyses indicated that the results did not change when participants' categorization performance on the post-experimental rating session was taken into account nor when only participants were included for whom the CFS mask was presented to the dominant eye.

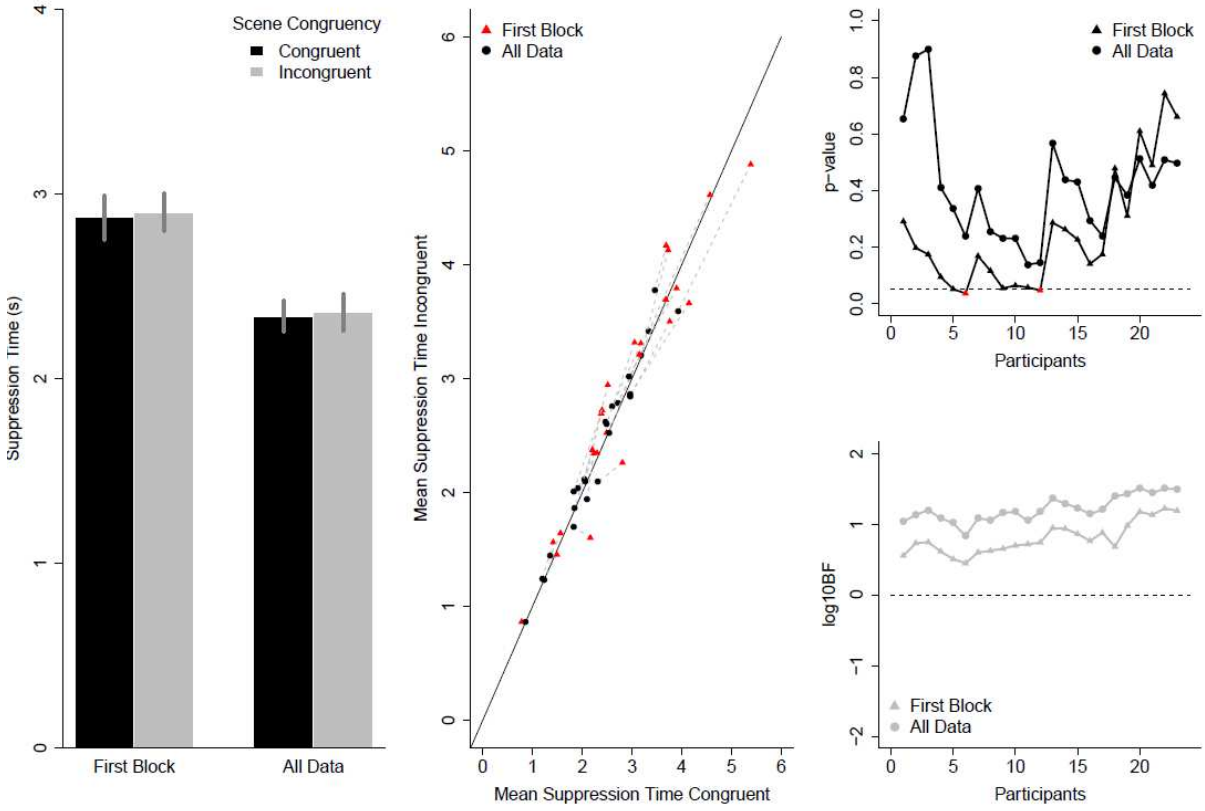

Figure 9.4. Results of Experiment 2. (left) Bar plot depicting mean suppression times for the first block only and all data. Error bars refer to 95% within-subject confidence intervals with the adjustment suggested by Morey (2008). (middle) Scatter plot of mean suppression times for congruent and incongruent scenes for each participant. Red triangles refer to the data of

the first block, black dots to all data together. The gray dashed lines connect the same participants. (right) Sequential analysis of the data. (top row) Evolution of p-values as more data were collected for the effect of scene congruency. Black symbols indicate p-values higher than .05 whereas red symbols indicate p-values smaller than .05. (lower row) Evolution of Bayes Factors. Bayes Factors higher than 0 indicate evidence for the absence of an effect whereas Bayes Factors smaller than 0 indicate evidence for the presence of an effect.

DISCUSSION

Although we did not obtain any evidence in favor of a scene congruency effect in both our experiments, one might still raise objections to our current attempts. That is, compared to the original study, we did not implement a post-experimental rating procedure in which participants were asked to categorize the images as being unusual or not (i.e., incongruent versus congruent). Second, the CFS mask was not presented in every participants' dominant eye. Third, the addition of an inversion condition in Experiment 1 and dropping the mask fade-out procedure as well as repeating the images more than once in Experiment 2 might have obscured a subtle congruency effect. We addressed these concerns in a third experiment, in which we presented every scene upright and only once across two experimental blocks and always in the participants' non-dominant eye. We included the mask fade-out procedure again, and implemented a post-experimental rating session. At this point, we invited participants from Experiments 1 and 2 to conduct this rating session on-line and reanalyzed the data based on their responses.

EXPERIMENT 3

METHODS

Participants

A new sample of 50 people participated in the experiment in return for money or course credit. All participants were naïve with respect to the design of the study and had normal or corrected-to-normal vision. All participants provided written informed consent prior to the start of the experiment.

Apparatus and Stimuli

The experimental set-up and stimuli were exactly the same as in Experiments 1 and 2.

Procedure and Design

Up to the post-experimental rating session, the procedure was exactly the same as in Experiments 1 and 2. Participants first completed the eye dominance experiment after which they completed two blocks of trials in which the scenes were presented to the participants non-dominant eye and the CFS mask to the dominant eye, according to the same procedure as in the previous experiments. After completing the CFS experiment, each scene was presented binocularly and participants had to indicate whether they thought the presented scene was unusual our not.

The experiment consisted of a within-subject design with two conditions (i.e., congruent versus incongruent scenes). In the main experiment, participants completed 8 practice trials, and 80 experimental trials. Within a block, the order in which trials were presented was completely randomized. The post-experimental rating session also consisted of 80 trials, presented in a random order.

RESULTS

The data of Experiment 3 were analyzed in the same way as in Experiments 1 and 2. That is, analyses were performed on correct trials (M =0.98, SD = 0.03) including only trials of which the suppression time did not exceed the time at which the mask reached 0% contrast (M = 0.91, SD = 0.18), after removing outliers defined as suppression times higher than the mean suppression time plus three times the standard deviation (for observers separately, M = 0.005, SD = 0.01). Furthermore, only stimuli that were correctly categorized after the experiment (i.e., congruent as incongruent and vice versa) were included in the analysis (M = 0.71, SD = 0.06). An analysis including the incorrectly rated scenes (see Supplementary Materials) did not change the results of the analysis.

The consistency of which stimuli broke suppression fast and slow (across observers) was similar to what was observed in Experiments 1 and 2 (α = 0.85). Furthermore, the ordering again correlated very strongly with the ordering observed in Experiment 1 (r = .89,

BF < 0.01) and Experiment 2 (r = .94, BF < 0.01), indicating that the images (irrespective of their congruency) were processed similarly across all three experiments.

As in Experiments 1 and 2, an analysis on mean correct suppression times did not reveal any effects of scene congruency ($M_{congruent}$ = 2.65, $M_{incongruent}$ = 2.66; $SD_{congruent}$ = 1.14, $SD_{incongruent}$ = 1.09; $t(49)$ = -0.15, p = .88, d = -0.02). Similarly, the BF analysis indicated strong evidence for the absence of a congruency effect (BF = 20). Figure 9.5 depicts the results of the sequential analysis. As is apparent from this figure, we observed a significant congruency effect in the classical analysis early on during data collection, yet the BF never crossed the boundary for indicating evidence in favor of a congruency effect and again gradually accumulated evidence in favor of the absence of a congruency effect.

In a supplementary exploratory analysis (see Supplementary Materials), we examined the relationship between various statistical image properties and the mean suppression times of the images in all three experiments. That is, given the absence of a scene congruency effect, yet high consistency of which images broke suppression slow and fast, we were interested in exploring whether some statistical properties of the images would predict suppression times. The results indicated that a measure of spatial coherence (an indicator of scene fragmentation) correlated positively with mean suppression duration. That is, when images were more cluttered, suppression times were on average higher.

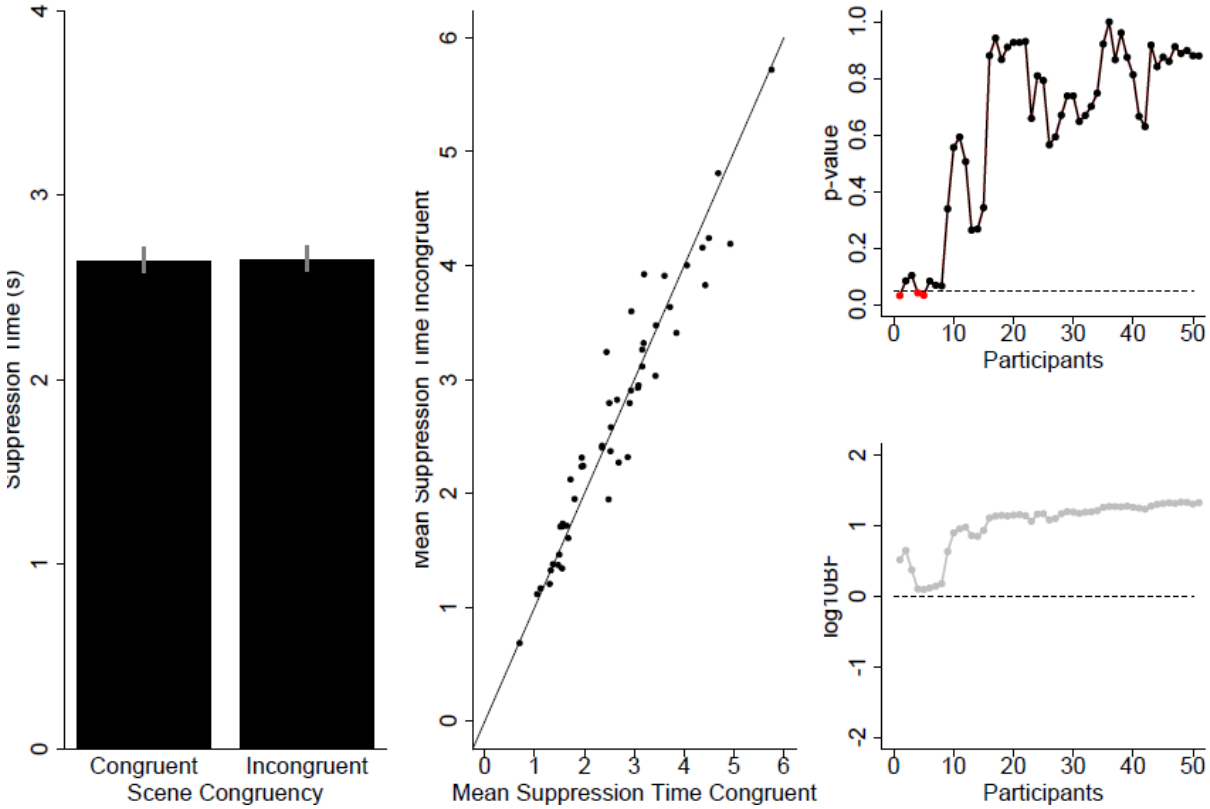

Figure 9.5. Results of Experiment 3. (left) Bar plot depicting mean suppression times for both conditions. Error bars refer to 95% within-subject confidence intervals with the adjustment suggested by Morey (2008). (middle) Scatter plot of mean suppression times for congruent and incongruent scenes for each participant. (right) Sequential analysis of the data. (top row) Evolution of p-values as more data were collected for the effect of scene congruency. Black symbols indicate p-values higher than .05 whereas red symbols indicate p-values smaller than .05. (lower row) Evolution of Bayes Factors. Log10BFs higher than 0 indicate evidence for the absence of a congruency effect whereas log10BFs smaller than 0 indicate evidence for the presence of a congruency effect.

DISCUSSION AND CONCLUSION

The goal of this study was to assess the replicability of the findings reported in Mudrik et al. (2011) and to gauge whether the scene congruency effect was attributable to low-level differences between the stimuli by including a scene inversion condition. In Experiment 1, using a larger sample as in the original study, we did not observe an effect of scene congruency, and critically, no interaction between scene inversion and scene congruency, highlighting that the inversion did not modulate the effect of scene congruency,

which would have been expected if the scene congruency effect was a semantic effect. Furthermore, a Bayes Factor (BF) analysis relying on linear mixed-effects models with crossed random effects showed convincing evidence for the absence of a scene congruency effect. A sequential analysis of our data highlighted a strong discrepancy between the inferences based on a classical repeated measures ANOVA and the BF analysis. Indeed, at several steps throughout our data collection, the classical analysis yielded a significant main effect of scene congruency whereas this was never the case for the BF analysis, indicating that not considering the random item variation yields a too liberal statistical procedure (Clark, 1973). Interestingly, in the case of the scene inversion effect, both the classical and the Bayesian analysis converged to the same conclusion. In Experiment 2, we increased the number of trials fourfold for each observer to assess whether increasing the precision of the effect size estimate for each observer would reveal a more consistent scene congruency effect. Again, we did not observe a scene congruency effect. Experiment 3 consisted of a third high-powered replication attempt in which we also included a post-experimental rating task to exclude stimuli that were not perceived as (in)congruent by our participants. The results of this last experiment also indicated strong evidence for the absence of a scene congruency effect.

What do the results of these experiments tell us? Could it be that the images were not processed at all? We contend that several aspects of our results argue against such an interpretation. In all experiments, we observed high internal consistency in which items broke suppression fast and slow across observers. Moreover, this pattern of slow and fast items correlated strongly across all experiments in independent sets of observers. Third, a measure of spatial coherence correlated with suppression times (with a similar magnitude) in all experiments. These aspects indicate that the scene stimuli did not break suppression in a completely random fashion.

In Experiment 1, we observed a consistent scene inversion effect. Could this be an indication that the stimuli were somehow interpreted during suppression? Although inversion reduces the identifiability of the scenes and preserves low-level image statistics, it may also influence higher-order image statistics to which the visual system is sensitive (Okazawa, Tajima, & Komatsu, 2015). Therefore, an inversion effect does not by definition

indicate that a stimulus is processed up to a semantic level, yet it could also reflect the sensitivity of the visual system to natural input.

Although the original scene congruency effect was interpreted as evidence for unconscious integration, our results cannot be interpreted as providing evidence *against* unconscious integration per se. That is, there remain some differences between our study and the original one. Besides the obvious differences in hardware, experimental environment, and pools of observers, there were also slight differences in our trial randomization procedure and the size of our CFS mask display. Our findings therefore show that the results of Mudrik et al. (2011) do not generalize across these particular testing differences, indicating that, if true, the scene congruency effect is particularly fragile. However, this lack of generalizability is hardly compatible with the conclusions derived from the original result. If unconscious integration can manifest under CFS, such an effect should not be dependent upon factors such as particular testing conditions or a different participant pool. Indeed, what our results show is that there is no evidence for scene integration without awareness under CFS. Indeed, although CFS initially proved to be a promising technique to assess the limits of unconscious visual processing (Bahrami et al., 2010; Jiang et al., 2006, 2007; Sklar et al., 2012), our findings fit well in a series of more recent findings highlighting rather limited visual processing during CFS (Hedger et al., 2015a; Hesselmann & Knops, 2014; Heyman & Moors, 2014; P. Moors, Huygelier, et al., 2015; P. Moors, Wagemans, et al., 2015). In hindsight, this is also not too surprising given that it is known that binocular rivalry disrupts processing of the suppressed stimulus beyond early visual areas (Fogelson et al., 2014; Hesselmann & Malach, 2011; Yuval-Greenberg & Heeger, 2013). This does not imply, however, that unconscious integration per se cannot take place. Indeed, some forms of unconscious integration have been shown to exist (see Mudrik, Faivre, & Koch, 2014 for a review), yet often relying on different suppression paradigms.

While this study highlights the importance of replication and the inclusion of appropriate control conditions, it also reveals a much broader point. That is, it provides an important example of how different statistical methods can strongly disagree throughout the data collection process. Indeed, although both types of analysis yielded the same conclusion at the end of data collection, it is important to highlight that the traditional repeated-measures ANOVA more than once indicated a significant scene congruency effect as the data

came in. The BF analysis, however, yielded a more consistent picture in that it always provided evidence for the absence of a scene congruency effect while the evidence also gradually accumulated when more data was collected. Furthermore, in Experiment 1, the BF indicated convincing evidence for the presence of an inversion effect and here, the results of the repeated measures ANOVA converged on those of the BF analysis. This highlights that for experimental designs in which the dependent measure can vary across participants and items, the classical repeated measures ANOVA approach might be too liberal (Clark, 1973) and an approach based on crossed random effects is recommended (Baayen et al., 2008).

In sum, our study questions the replicability and generalizability of the findings reported in Mudrik et al. (2011) by obtaining strong evidence for the absence of a scene congruency effect across three experiments and moreover showing that scene congruency was not modulated by scene inversion. Therefore, it is unlikely that during CFS, complex high-level scene processing can ensue.

AUTHOR CONTRIBUTIONS

P. Moors developed the study concept. Testing and data collection were performed by D. Boelens and J. van Overwalle. P. Moors performed the data analysis and interpretation. P. Moors and J. Wagemans drafted the manuscript and D. Boelens and J. van Overwalle provided critical revisions. All authors approved the final version of the manuscript for submission.

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SUPPLEMENTARY MATERIALS

In this Supplementary Material, we report on additional analyses for Experiments 1 and 2 as well as an exploratory analysis on the relationship between mean suppression times obtained for each image averaged across observers and four statistical properties of the images.

In Experiment 3, we implemented a post-experimental rating procedure in which participants had to indicate whether they thought a presented scene was unusual or not. This procedure was not implemented in Experiments 1 and 2 and might have influenced the results. Therefore, we invited the participants of the first two experiments to participate in an on-line experiment in which they had to rate all 80 scenes. The data of the first two experiments were then reanalyzed by first excluding incorrectly rated scenes (i.e., an incongruent scene was rated as congruent and vice versa). Second, in Experiment 3, a technical issue was also fixed. That is, due to a bug in the code, after the eye dominance measurement, the CFS mask was always presented to the right eye rather than the dominant eye. For Experiments 1 and 2, we also reanalyzed the data by excluding participants for which the CFS mask was not presented to the dominant eye (i.e., participants for whom the dominant eye was the left one).

Supplementary analysis for Experiment 1

23 out of 45 (51%) participants responded to our invitation to participate in the rating experiment. Figures S9.1 and S9.2 depict the results of Experiment 1 for these 23 participants after also excluding all data points from incorrectly rated scenes. The results are very similar to those observed when analyzing the full data set. That is, a two-way repeated measures ANOVA on the mean correct suppression times revealed no main effect of scene congruency ($M_{congruent}$ = 2.36, $M_{incongruent}$ = 2.35; $SD_{congruent}$ = 0.94, $SD_{incongruent}$ = 0.88; $F(1,22) = 0.06$, $p = .81$, $d = 0.04$), a main effect of scene inversion ($M_{upright}$ = 2.31, $M_{inverted}$ = 2.40; $SD_{upright}$ = 0.92, $SD_{inverted}$ = 0.90; $F(1,22) = 5.52$, $p = .03$, $d = 0.38$), and no interaction between scene congruency and scene inversion ($F(1,22) = 0.082$, $p = .78$, $d = -0.06$). The Bayes Factor analysis indicated strong evidence in favor of the absence of a scene congruency effect (BF = 22), moderate evidence in favor of the presence of a scene inversion effect (BF = 6) and strong evidence for the absence of an interaction effect (BF = 16).

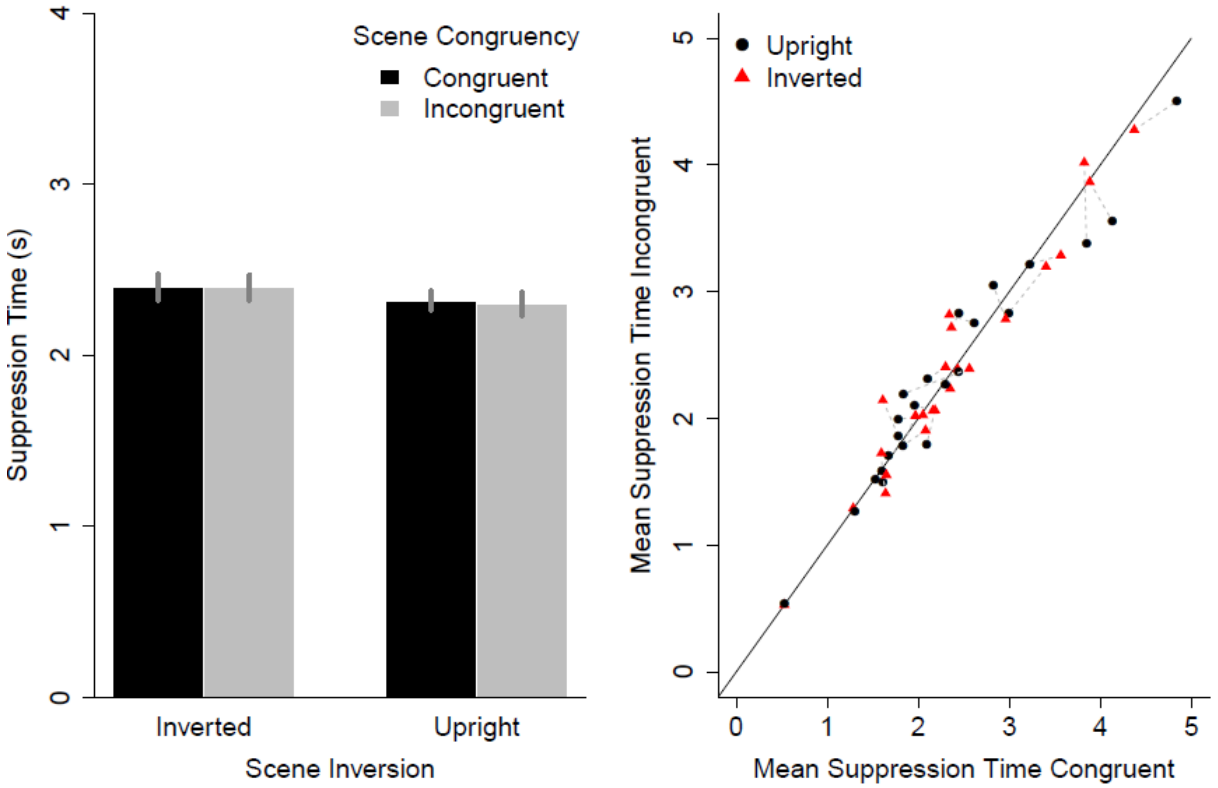


Figure S9.1.

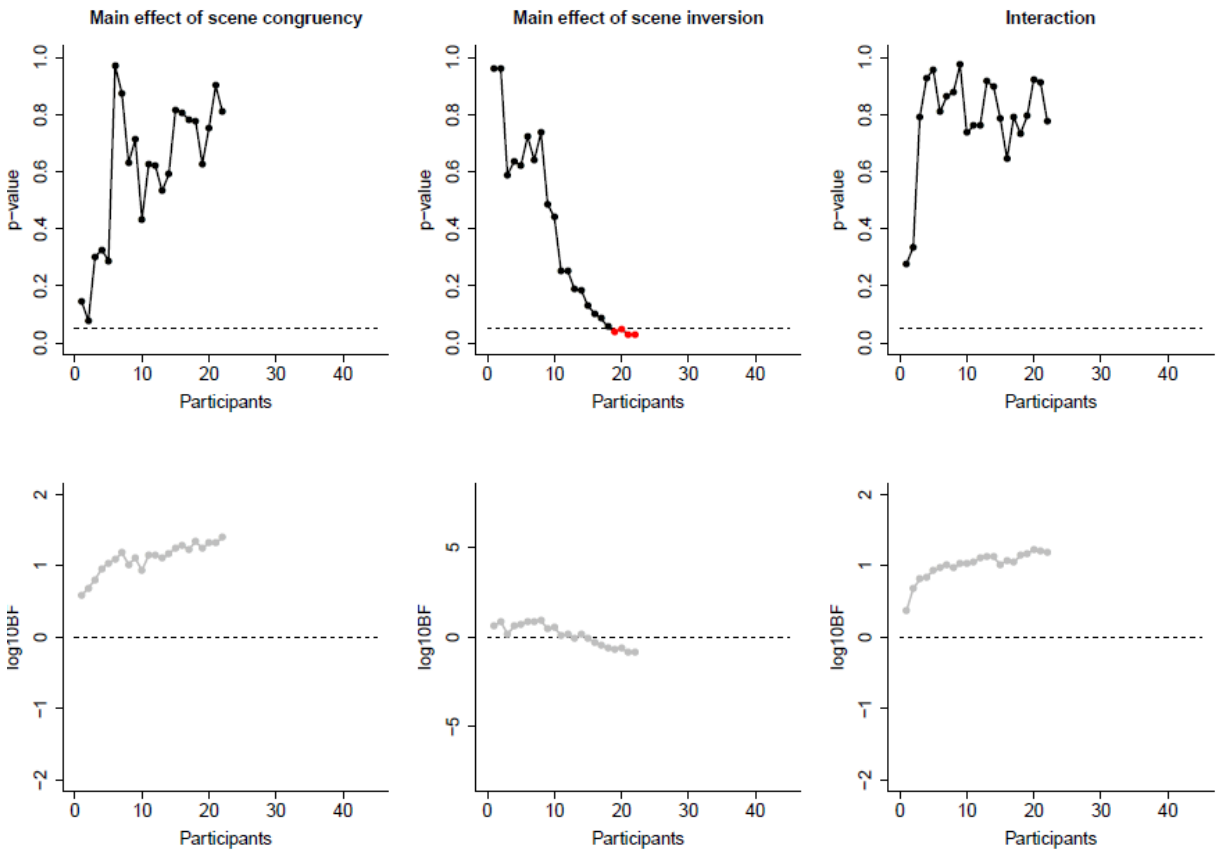


Figure S9.2

We conducted the same analysis, but now excluding all participants for which the CFS mask was not presented to the dominant eye. This reduced our sample to 25 participants (20 were excluded). The results of this analysis are depicted in Figures S9.3 and S4. Again, the results are very similar to those observed when analyzing the full data set. That is, a two-way repeated measures ANOVA on the mean correct suppression times revealed no main effect of scene congruency ($M_{congruent}$ = 2.73, $M_{incongruent}$ = 2.67; $SD_{congruent}$ = 1, $SD_{incongruent}$ = 0.97; $F(1,24) = 2.94$, $p = .1$, $d = 0.20$), no main effect of scene inversion ($M_{upright}$ = 2.67, $M_{inverted}$ = 2.73; $SD_{upright}$ = 1.01, $SD_{inverted}$ = 0.96; $F(1,24) = 2.13$, $p = .16$, $d = 0.20$), and no interaction between scene congruency and scene inversion ($F(1,22) = 0.05$, $p = .82$, $d = -0.05$). The Bayes Factor analysis indicated evidence in favor of the absence of a scene congruency effect (BF = 6), evidence in favor of the presence of a scene inversion effect (BF = 4) and strong evidence for the absence of an interaction effect (BF = 17).

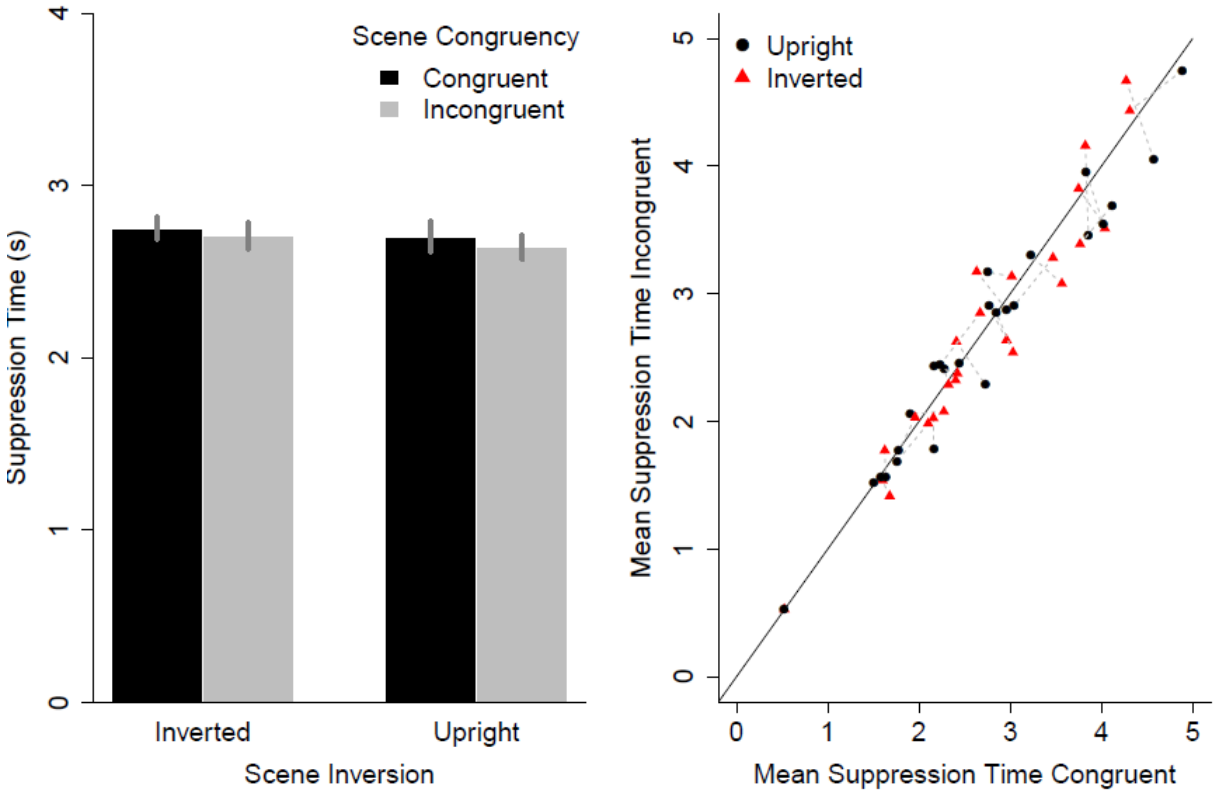


Figure S9.3.

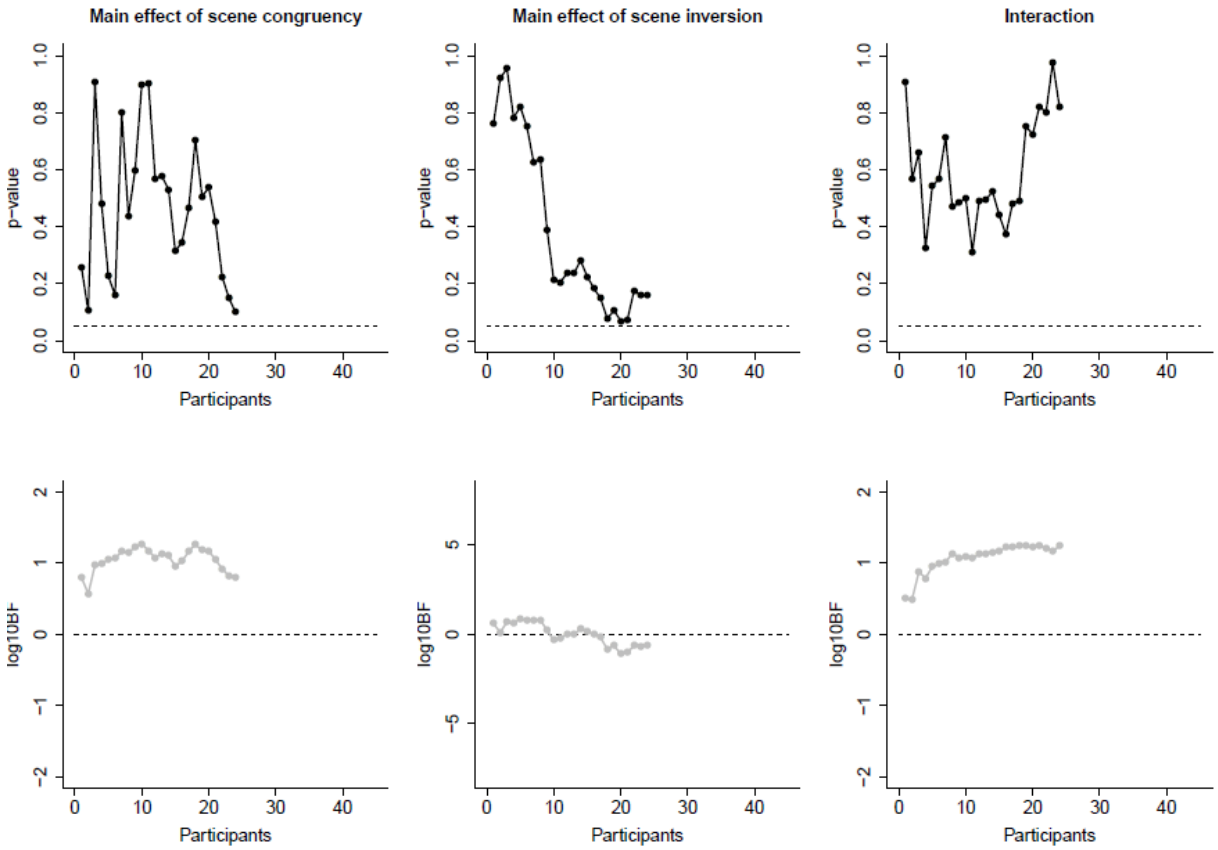


Figure S9.4.

For the sake of completeness, including only those people that responded to the invitation to participate in the rating experiment and those for which the CFS mask was presented to the dominant eye further reduced our sample to 15 participants. This analysis yielded slightly different results compared to the analysis of the full data set and is depicted in Figures S9.5 and S9.6. That is, a two-way repeated measures ANOVA on the mean correct suppression times revealed a main effect of scene congruency ($M_{congruent}$ = 2.53, $M_{incongruent}$ = 2.43; $SD_{congruent}$ = 1.09, $SD_{incongruent}$ = 1.03; $F(1,14) = 17.06$, $p = .001$, $d = 0.59$), no main effect of scene inversion ($M_{upright}$ = 2.44, $M_{inverted}$ = 2.52; $SD_{upright}$ = 1.08, $SD_{inverted}$ = 1.05; $F(1,14) = 3.38$, $p = .09$, $d = 0.38$), and, critically, no interaction between scene congruency and scene inversion ($F(1,14) = 0.38$, $p = .55$, $d = -0.16$). The Bayes Factor analysis did not indicate a preference for the presence or absence of a congruency effect (BF = 1.3), indicated evidence in favor of the presence of a scene inversion effect (BF = 4) and indicated strong evidence for the absence of an interaction effect (BF = 7).

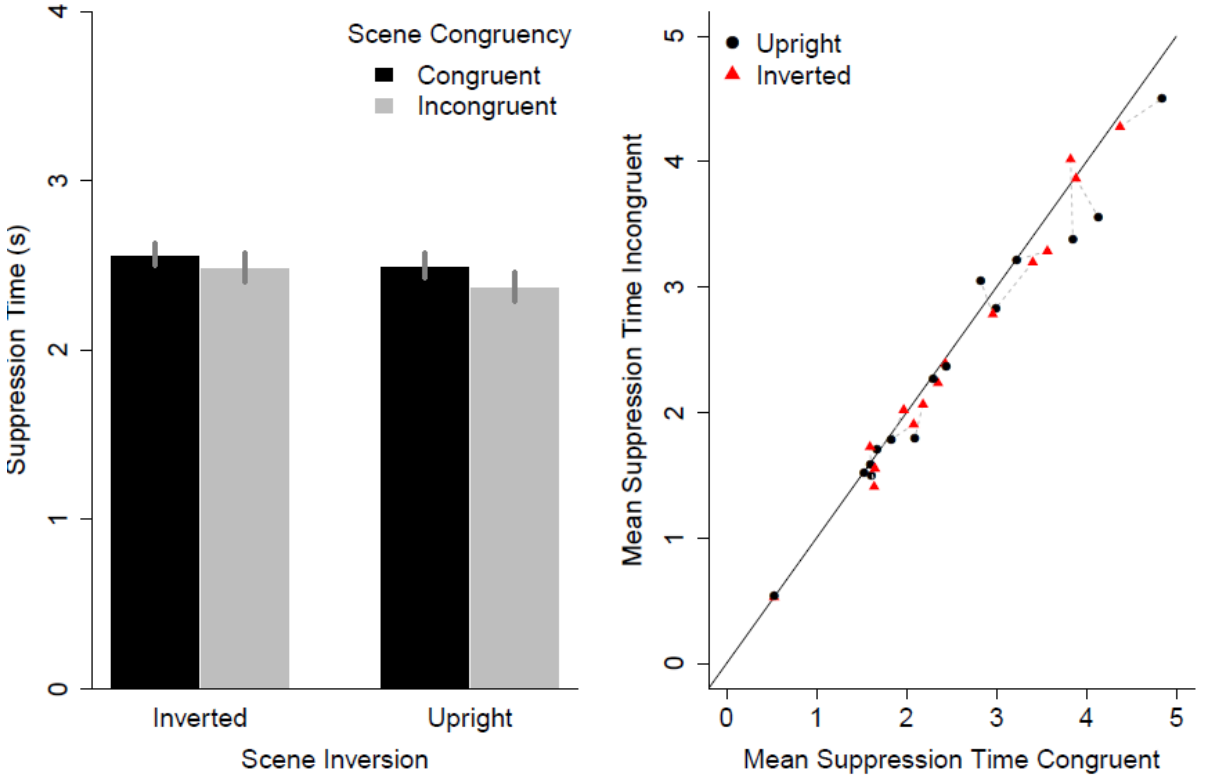


Figure S9.5.

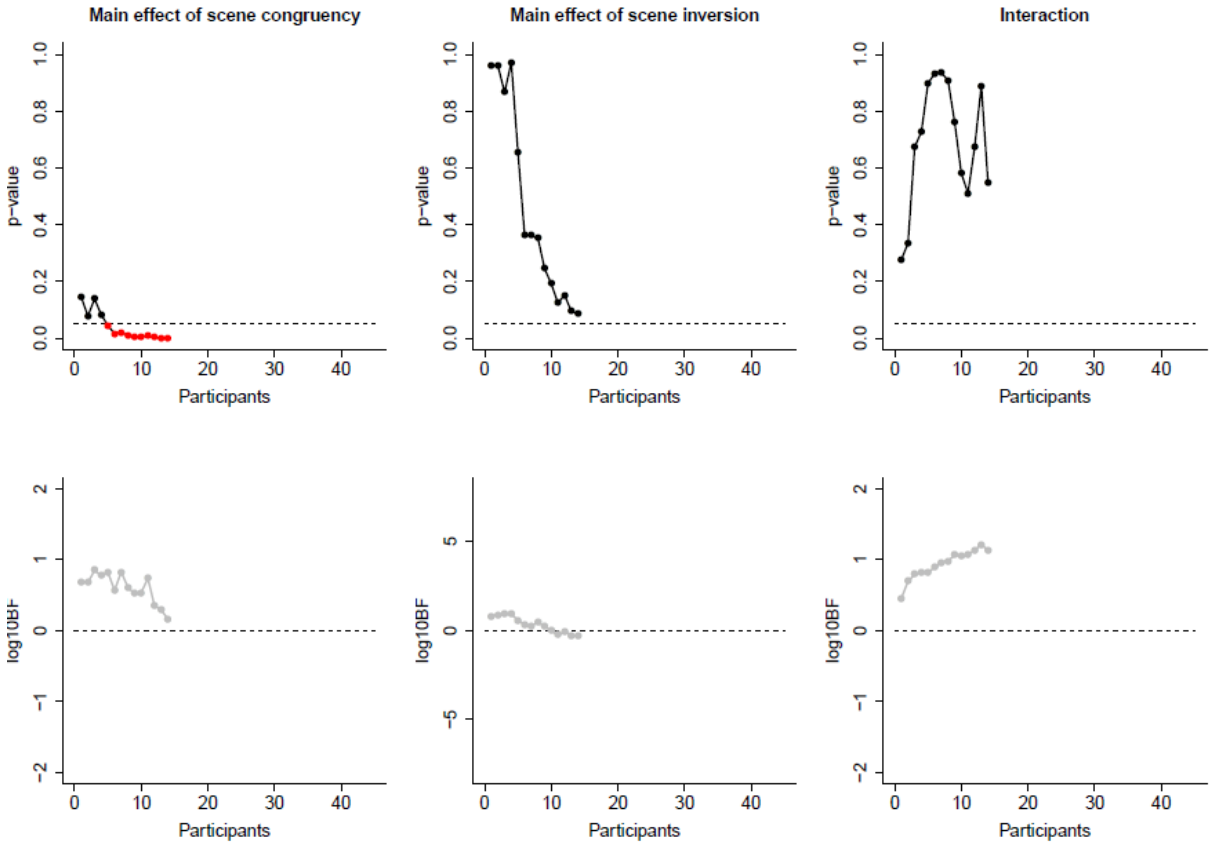


Figure S9.6.

Supplementary analysis for Experiment 2

We now report on the same reanalysis of the data of Experiment 2 by first considering those participants that performed the rating experiment, then considering those for which the CFS mask was presented in the dominant eye, and ending with combining both exclusion criteria.

18 out of 24 (75%) participants responded to our invitation to participate in the rating experiment. Excluding all incorrectly rated scenes yielded a similar picture as the results for the full data set. That is, no effect of scene congruency was observed neither in the first block ($M_{congruent}$ = 2.73, $M_{incongruent}$ = 2.81; $SD_{congruent}$ = 1.03, $SD_{incongruent}$ = 0.95; $t(17)$ = -1.42, p = .17, d = -0.34) nor when considering the data as a whole ($M_{congruent}$ = 2.23, $M_{incongruent}$ = 2.28; $SD_{congruent}$ = 0.71, $SD_{incongruent}$ = 0.70; $t(17)$ = -1.79, p = .09, d = -0.42). Similarly, a BF analysis of the data always indicated convincing evidence for the absence of a scene congruency effect (BF = 6 and BF = 14, for the first block and all data, respectively). Figure S9.7 depicts the results of this analysis.

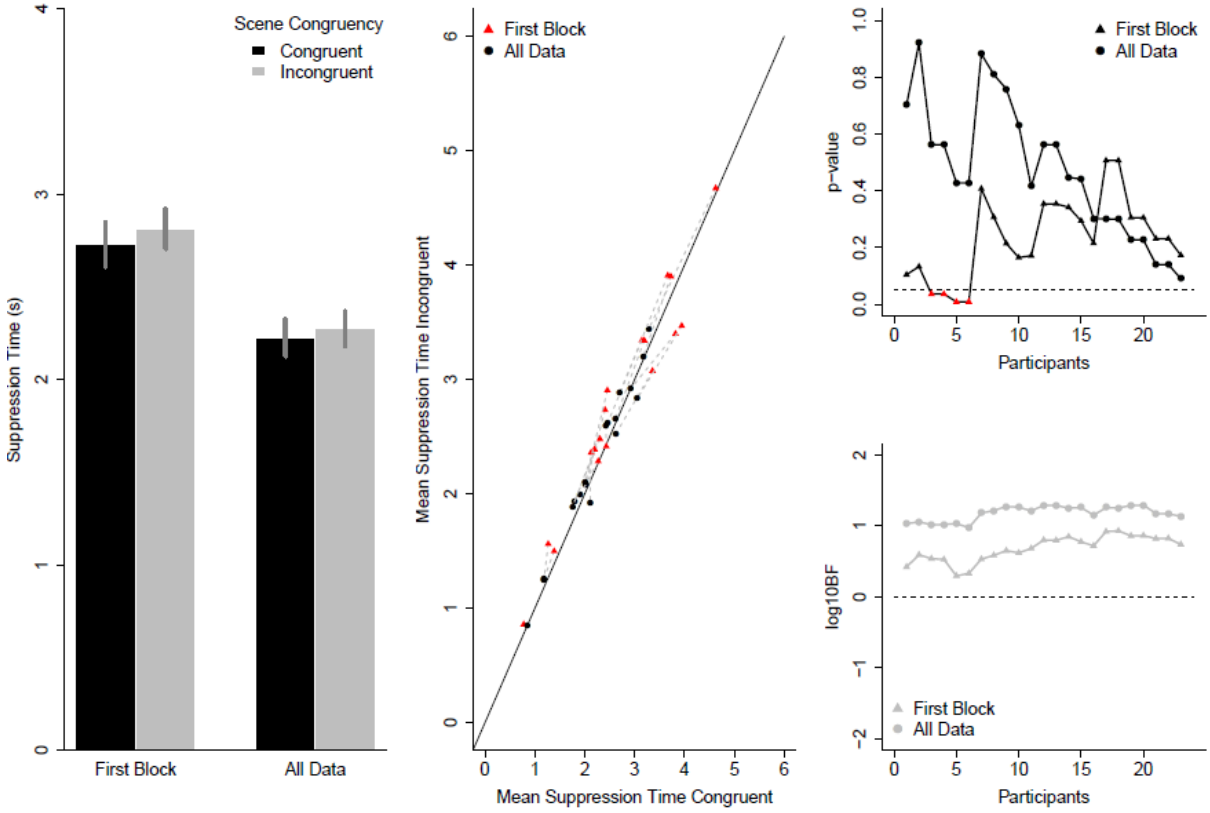


Figure S9.7.

Including only those participants for which the CFS mask was presented to the dominant eye reduced our sample to 12 observers. The results are summarized in Figure S9.8 and are very similar to the analyses for the full data set. That is, no effect of scene congruency was observed neither in the first block ($M_{congruent}$ = 3.47, $M_{incongruent}$ = 3.53; $SD_{congruent}$ = 0.93, $SD_{incongruent}$ = 0.91; $t(11)$ = -0.41, p = .69, d = -0.12) nor when considering the data as a whole ($M_{congruent}$ = 2.77, $M_{incongruent}$ = 2.80; $SD_{congruent}$ = 0.67, $SD_{incongruent}$ = 0.65; $t(11)$ = -0.53, p = .61, d = -0.15). Similarly, a BF analysis of the data always indicated convincing evidence for the absence of a scene congruency effect (BF = 13 and BF = 26, for the first block and all data, respectively).

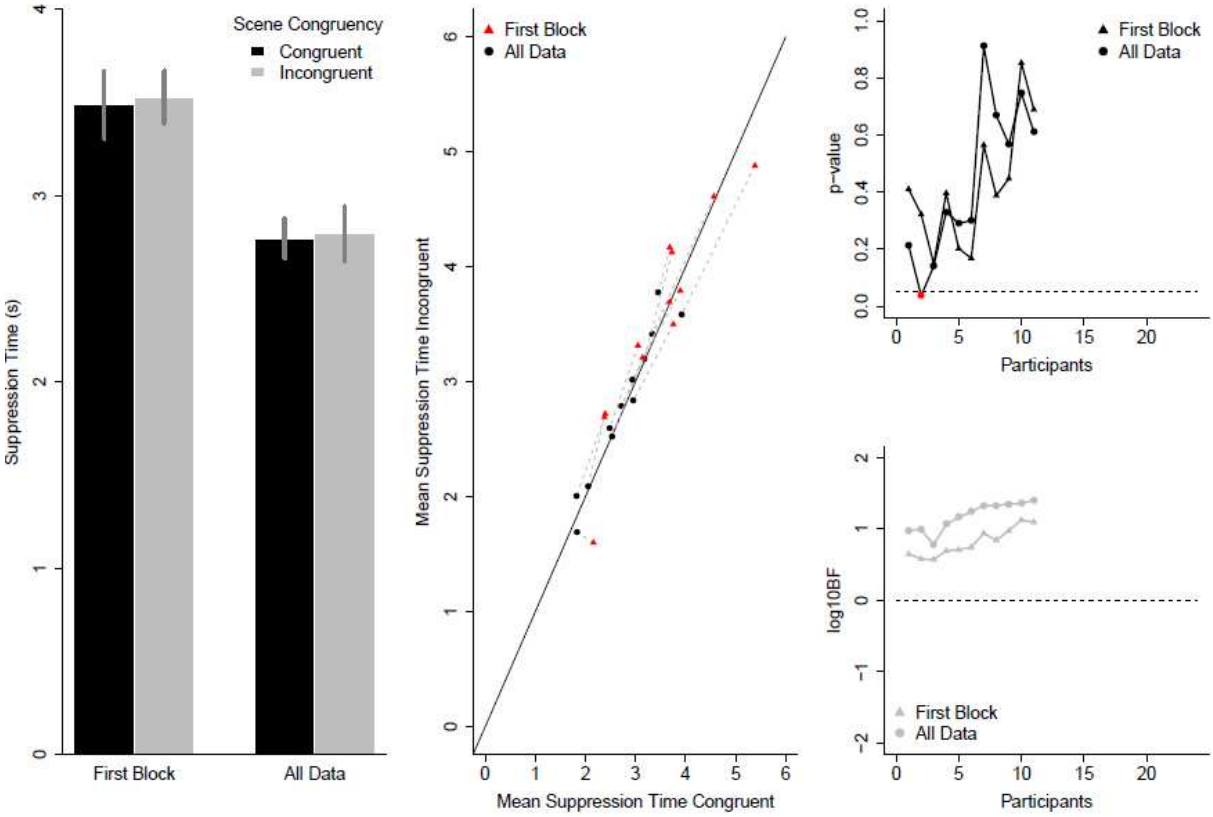
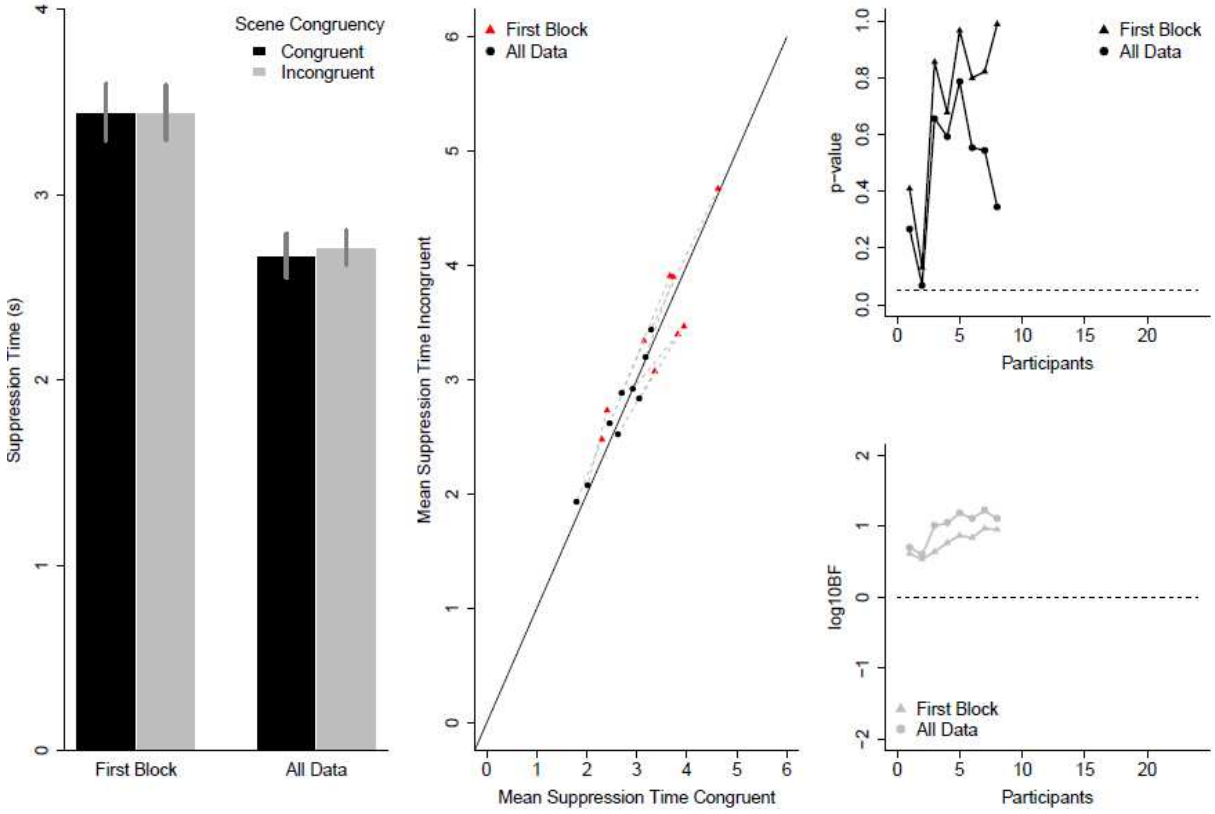


Figure S9.8.

Last, an analysis combining both exclusion criteria further reduced our sample to 9 participants. The results of this analysis are summarized in Figure S9.9. Again, these results are very similar to those observed for the full data set. No effect of scene congruency was observed neither in the first block ($M_{congruent}$ = 3.45, $M_{incongruent}$ = 3.44; $SD_{congruent}$ = 0.74, $SD_{incongruent}$ = 0.66; $t(8)$ = 0.01, p = .99, d = 0.005) nor when considering the data as a whole ($M_{congruent}$ = 2.67,

$M_{incongruent}$ = 2.72; $SD_{congruent}$ = 0.51, $SD_{incongruent}$ = 0.49; $t(8) = -1$, $p = .35$, $d = -0.33$). Similarly, a BF analysis of the data always indicated convincing evidence for the absence of a scene congruency effect (BF = 9 and BF = 13, for the first block and all data, respectively).



Supplementary analysis for Experiment 3

For Experiment 3, we reported an analysis of the mean suppression times after removing incorrect responses during the main experiment as well as incorrectly categorized stimuli during the post-experimental rating session. Here, we report on the same analysis, yet including the stimuli that were incorrectly categorized during the post-experimental rating session. The results of this analysis were very similar to those excluding the incorrectly categorized stimuli (see Figure S9.10). No effect of scene congruency was observed ($M_{congruent}$ = 2.64, $M_{incongruent}$ = 2.63; $SD_{congruent}$ = 1.12, $SD_{incongruent}$ = 1.06; $t(49) = 0.11$, $p = .91$, $d = .016$). Similarly, the BF analysis indicated convincing evidence for the absence of a congruency effect (BF = 24).

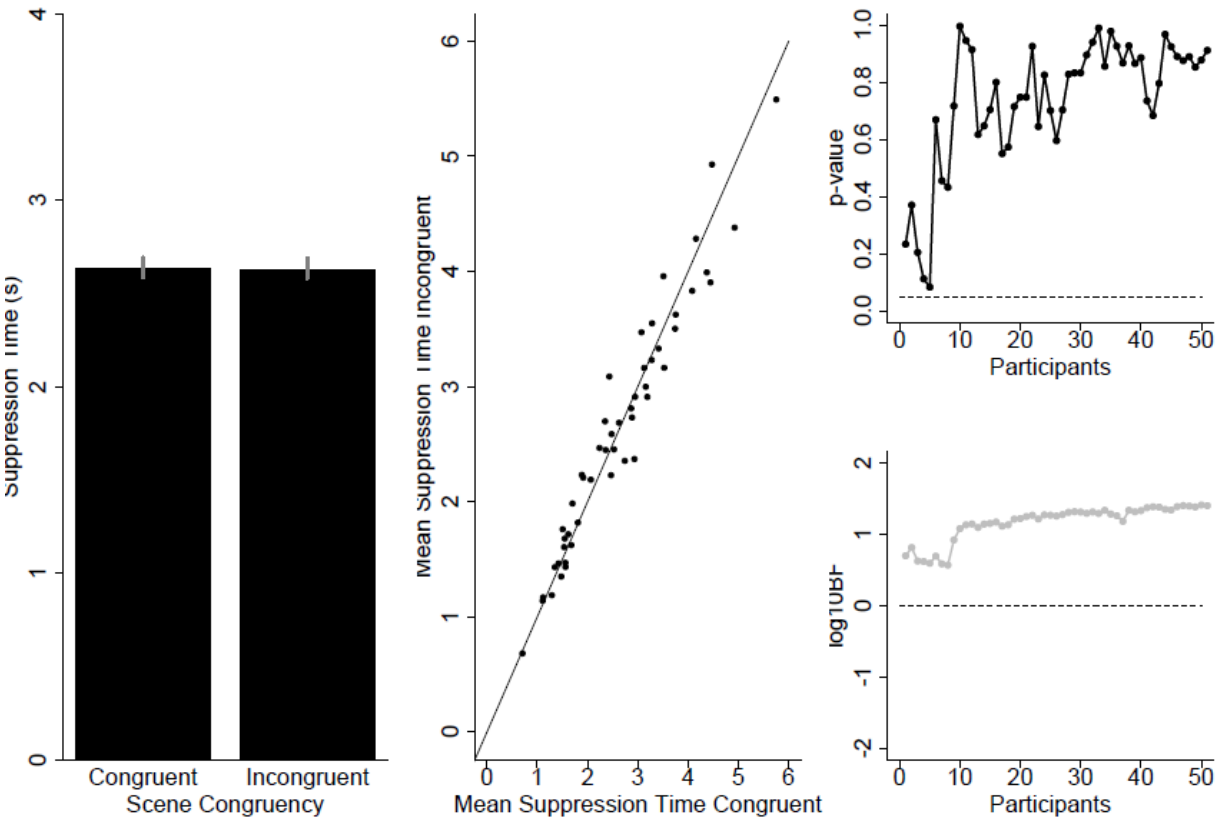


Figure S9.10.

Correlation between suppression times and statistical properties of the images

A final exploratory analysis pertains to the relationship between mean suppression times obtained for each image averaged across observers and four statistical properties of the images. These four statistical measures include the intercept (IC) and slope (SL) derived from a regression line fitted to the Fourier amplitude spectrum of each image in log-log space. Furthermore, for each image, we obtained measures of contrast energy (CE) and spatial coherence (SC) as reported in Groen, Ghebreab, Prins, Lamme, and Scholte (2013). These measures involve approximations of a Weibull fit to the histograms of local contrast filter responses. Note that high values of SC indicate cluttered images, whereas low values indicate images that are spatially coherent.

Figure S9.11 depicts the correlation matrices for all measures considered and mean suppression time (ST) for each item, averaged across observers and scene congruency. For all experiments, the highest correlation of interest that was observed was the one between suppression time and spatial coherence (Experiment 1: r = 0.45; Experiment 2: r = 0.53; Experiment 3: r = 0.49). This correlation was also the only one (of the ones that included

suppression time) that exceeded a Bayes Factor of 3 for all experiments (Wetzels & Wagenmakers, 2012).

This supplementary exploratory analysis indicates that the observed suppression durations correlate with a measure of spatial coherence of the images, indicating that cluttered images on average yield slower suppression times compared to spatially coherent, less fragmented images.

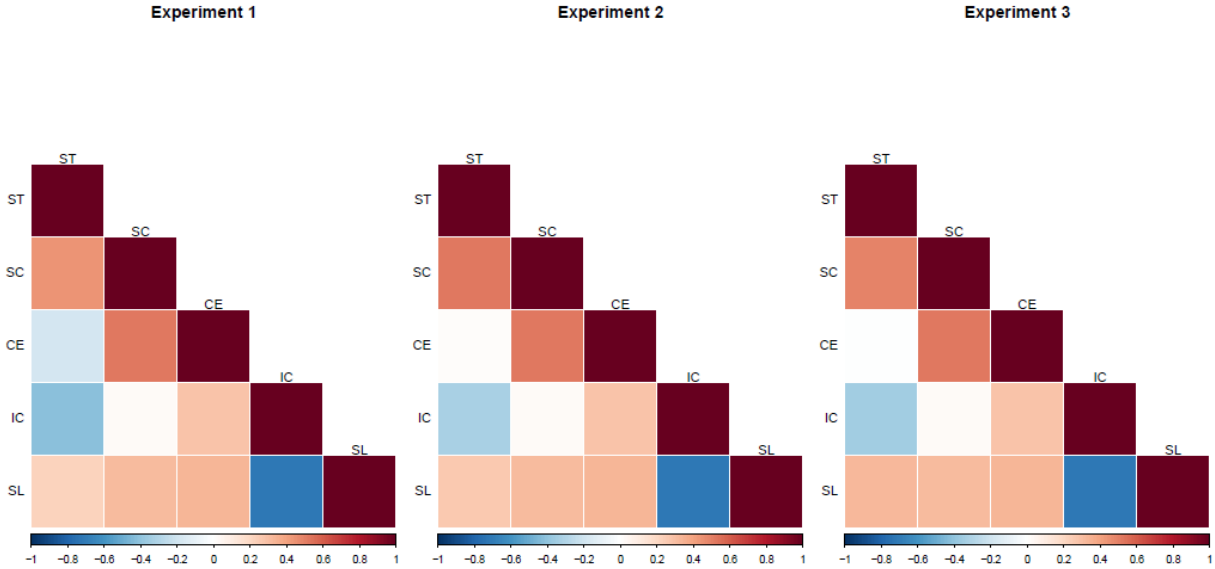


Figure S9.11. Relationship between suppression times and image characteristics. Correlation matrices for all four measures and suppression time, for both experiments. ST = suppression time, SC = spatial coherence, CE = contrast energy, IC = intercept, SL = slope.

Table S9.1. Bayes Factors for correlations between suppression time and each image statistic.

Statistic	Experiment 1	Experiment 2	Experiment 3
SC	7.6	53	21
CE	0.23	0.12	0.12
IC	4.2	0.97	1.27
SL	0.32	0.49	0.97

Note. Bayes Factors > 1 indicate evidence for a correlation being different from zero.

Chapter 10.
Frequent words do not break continuous flash suppression differently from infrequent or nonexistent words: Implications for semantic processing of words in the absence of awareness.

Continuous flash suppression (CFS) has been used as a paradigm to probe the extent to which word stimuli are processed in the absence of awareness. In the two experiments reported here, no evidence is obtained that word stimuli are processed up to the semantic level when suppressed through CFS. In Experiment 1, word stimuli did not break suppression faster than their pseudo-word variants nor was suppression time modulated by word frequency. Experiment 2 replicated these findings, but more critically showed that differential effects can be obtained with this paradigm using a simpler stimulus. In addition, pixel density of the stimuli did prove to be related to suppression time in both experiments, indicating that the paradigm is sensitive to differences in detectability. A third and final experiment replicated the well-known face inversion effect using the same set-up as Experiments 1 and 2, thereby demonstrating that the employed methodology can capture more high-level effects as well. These results are discussed in the context of previous evidence on unconscious semantic processing and two potential explanations are advanced. Specifically, it is argued that CFS might act at a level too low in the visual system for high-level effects to be observed or that the widely used breaking CFS paradigm is merely ill-suited to capture effects in the context of words.

Heyman, T., Moors, P. (2014). Frequent Words Do Not Break Continuous Flash Suppression Differently from Infrequent or Nonexistent Words: Implications for Semantic Processing of Words in the Absence of Awareness. *PLoS One*, 9 (8), art.nr. e104719.

INTRODUCTION

Although our visual awareness of the world most of the time is continuous and stable, sometimes conscious perception fluctuates while retinal input stays constant. Amongst other, this situation arises when the two eyes are presented with different stimuli at corresponding retinal locations. Instead of mixing the signals of both eyes based on, for example, a weighted sum, the visual system appears to "decide" to categorically favor the image presented to one eye or the other and to stochastically alternate between the two interpretations, a phenomenon known as binocular rivalry. Since conscious perception fluctuates while visual input does not change, binocular rivalry has been proposed as a technique to study the (neural) correlates of consciousness (Blake & Logothetis, 2002; Kim & Blake, 2005; Tong et al., 2006). However, the stochastic nature of the rivalry process made it hard for researchers to reliably suppress stimuli for a time period that allowed them to measure the extent to which these suppressed stimuli were still processed in the absence of awareness. Continuous flash suppression (CFS) proved to be a solution to this problem by introducing a repetitive, dynamic signal in one eye which seemed to more effectively suppress the stimulus presented in the other eye (Tsuchiya & Koch, 2005; Tsuchiya, Koch, Gilroy, & Blake, 2006). Upon its introduction, CFS was rapidly picked up on as a reliable technique to study unconscious processing of various classes of stimuli. One of these research lines pertained to whether semantic information of words is extracted in the absence of awareness. To study unconscious semantic processing of words, the breaking CFS paradigm (b-CFS, Stein, Hebart, & Sterzer, 2011) has been mostly used. In b-CFS, a stimulus is presented in one eye (usually at low contrast) and a CFS mask in the other. Due to its high contrast and dynamic nature, the CFS mask dominates initially. The contrast of the other stimulus is then gradually increased until it "breaks through" the CFS mask. The time until breakthrough (i.e., suppression time) is commonly used as an index of unconscious processing of the stimulus. That is, if different stimuli break CFS on average differentially, it is assumed that some kind of unconscious representation must have biased the breaking through CFS (e.g., see Jiang, Costello, & He, 2007).

Upon reviewing the literature, it became apparent that some conflicting findings had been reported with respect to the unconscious processing of semantic information of words. For example, Costello, Jiang, Baartman, McGlennen, and He (2009) observed that

suppression times of words were influenced by the prime-target relation of a previously presented visible prime word. That is, when a semantically related prime preceded the suppressed target, it broke suppression faster than when prime and target were unrelated. Seemingly in contrast, Sklar et al. (2012) found that short semantically incongruent sentences broke suppression *faster* than semantic congruent sentences.

Another line of research pertains to the question whether semantic information of emotional words is extracted in the absence of awareness. To address this question, Yang and Yeh (2011) presented participants with neutral and negatively valenced Chinese two-character words. They observed that negative words take longer to break suppression than neutral words. In apparent contradiction with the findings of Yang and Yeh (2011), Sklar et al. (2012) report on experiments in which a negatively valenced combination of two neutral words (e.g., black eye) broke suppression faster than a neutral combination of two neutral words.

In sum, no consistent pattern of findings has emerged from the studies on unconscious semantic processing of words. For semantic congruency relations as well as for negatively valenced words or word relations, studies disagree as to whether such stimuli break suppression slower or faster. It is noteworthy that these studies all addressed relatively specific questions regarding unconscious processing of *words*. However, it has not been clearly established that words indeed have a special status. That is, no study has yet probed whether a difference would be observed between suppression times of words and non/pseudo-words presented under CFS. Secondly, we sought to assess whether the word frequency effect, one of the most robust findings in the psycholinguistic literature (e.g., Keuleers, Lacey, Rastle, & Brysbaert, 2012), would manifest itself under CFS. That is, visual word recognition occurs faster for highly frequent words. Here, we investigated whether suppression times of words also correlate with their respective word frequency. In our first experiment, we set out to test both hypotheses. That is, we generated a set of words varying in word frequency and an associated set of pseudo-words. These stimuli were presented under CFS and participants had to indicate the position of the suppressed stimulus upon breakthrough (i.e., either below or above a fixation cross). To preview our results, we found no evidence for differential suppression times between words and pseudo-words nor a correlation between word frequency and suppression time.

EXPERIMENT 1

Materials and Methods

Ethics Statement

The study was conducted in line with the ethical principles regarding research with human participants as specified in The Code of Ethics of the World Medical Association (Declaration of Helsinki). The study was approved by the Ethical Committee of the Faculty of Psychology and Educational Sciences (EC FPPW) of the University of Leuven, and the participants gave written informed consent before starting the experiment.

Participants

Eighteen healthy subjects (6 male, age range 18 – 30 years) volunteered for the experiment and were paid for their participation. All participants had normal or corrected-to-normal vision and were naïve with respect to the goal of the study.

Apparatus

Stimuli were shown on two 19.8-in. Sony Trinitron GDM F500-R (2048 x 1536 pixels at 60 Hz, for each) monitors driven by a DELL Precision T3400 computer with an Intel Core Quad CPU Q9300 2.5 GHz processor running on Windows XP. Binocular presentation was achieved by a custom made stereo set-up. Two CRT monitors, which stood opposite to each other (distance of 220 cm), projected to the left and right eye respectively via two mirrors placed at a distance of 110 cm from the screen. A vertical plate ensured stable projection from the left and right screen to the left and right eye, respectively. A head- and chin rest (15 cm from the mirrors) were used to stabilize fixation. The effective viewing distance was thus 125 cm. Stimulus presentation, timing and keyboard responses were controlled with custom software programmed in Python 2.7 using the PsychoPy library (Peirce, 2007, 2009).

Stimuli

A random checkerboard pattern was used as the background display to achieve stable binocular fusion. The individual elements of the checkerboard subtended 0.34° by 0.34°. For stimulus presentation, two grey frames were superimposed on the checkerboard

pattern (frame size 10° by 10°). A white fixation cross (0.6° by 0.6°) was continuously present during the experiment.

The main experiment was preceded by an eye dominance measurement phase in which the target was an arrow (maximal width 4°; maximal height 2°). For measuring eye dominance, the CFS mask consisted of 150 squares with a randomly picked size (between 1° and 2° width) and a random grayscale value on each refresh of the mask. In the main experiment, the size of the individual elements of the CFS mask ranged between 0.2° and 1.2°. The mask contained 200 squares with a randomly picked color on each refresh. In all the phases of the experiments reported here, the refresh rate of the CFS mask was set to 10Hz.

A total of 154 Dutch words were selected from the SUBTLEX-NL database, which, as a whole, showed a word frequency effect on lexical decision latencies and accuracies (Keuleers, Brysbaert, & New, 2010). Word frequency was operationalized as the log-transformed number of contexts in which a word occurs (Brysbaert & New, 2009) and ranged from 0.669 to 3.882 (see Table 10.1 for a summary of the stimulus characteristics). The word stimuli were then used as input for Wuggy, a program that generates pronounceable pseudo-words (Keuleers & Brysbaert, 2010). Thus, each word had an orthographically similar pseudo-word counterpart (e.g., *lamp* – *hamp*). The size of the words ranged from 0.92° to 3.9° depending on the length of the word, which varied from two to seven letters. The height of the words was maximally 0.92°. In addition to word length, we also derived a measure of pixel density by summing all pixels comprising each stimulus. Furthermore, we obtained more high-level characteristics such as age of acquisition (i.e., an estimate of the age at which a word has been learned) and concreteness (i.e., an estimate of how concrete a concept is) from Moors et al. (2013) and Brysbaert, Stevens, De Deyne, Voorspoels, and Storms (2014).

Table 10.1. Descriptive Statistics for the Words in Experiment 1 and 2.

Factor	Mean (*SD*s in parentheses) for Experiment 1	Mean (*SD*s in parentheses) for Experiment 2
Word Frequency	2.33 (0.74)	2.29 (0.76)
Word Length	4.22 (0.83)	4.57 (0.98)
Pixel Density	5,364 (1,130)	5,799 (1,468)
Age of Acquisition	7.39 (2.23)	7.37 (2.13)
Concreteness	4.14 (0.87)	4.11 (0.90)

Note. Word Frequency is the log-transformed number of contexts in which a certain word occurs (Keuleers, Brysbaert, et al., 2010). Word Length is the number of characters. Pixel Density refers to the sum of all pixels that comprised the stimulus. Age of acquisition is the estimated age in years at which a word is learned (Brysbaert et al., 2014; Moors et al., 2013). Concreteness is an estimate on a five-point likert scale of how concrete a concept is (the higher, the more concrete) (Brysbaert et al., 2014). Age of acquisition and concreteness estimates were not available for one word in both Experiment 1 and 2.

Procedure

Prior to the start of the main experiment, participants' eye dominance was measured according to the method of Yang, Blake, and McDonald (2010). On every trial, participants were presented with an arrow in one eye gradually increasing contrast from 0 to 100% and pointing either left or rightwards. In the other eye, the CFS mask was presented. As soon as the arrow broke suppression, participants had to indicate its direction by pressing 1 or 3 on the numerical keyboard for the left and right direction, respectively. Subsequently, eye dominance was determined by comparing the average suppression times of the left eye to that of the right. The eye for which the average suppression time was the lowest was considered to be the dominant eye. Consequently, the CFS mask was presented in this eye throughout the rest of the experiment.

In the main experiment, the word or pseudo-word stimuli were presented in lower case letters either 2° above or below the fixation cross and gradually faded in from 0 to 50% contrast over a period of 2 seconds (see Figure 10.1). Upon breakthrough, participants had to indicate as fast as possible the location of the stimulus (above or below fixation) with a

button press on the numerical keyboard (1 for above, 3 for below), initiating a new trial. A fixation cross was presented during the intertrial interval, which lasted 2 seconds.

Design

During the eye dominance measurement phase, participants completed 80 trials in total, in half of which the target was presented to the left eye. For each eye, half of the targets pointed leftwards. Trial presentation was randomized.

The main experiment consisted of 308 trials, 154 word trials and 154 pseudo-word trials, split up in two blocks between which participants took a break of at least one minute. Besides the word type manipulation, we also used word stimuli that varied in frequency of occurrence. To ensure that one element of a word - pseudo-word pair (e.g., *lamp*) could not prime the other (e.g., *hamp*), they were always presented in different blocks. That is, the words of a random half of the pairs were presented in the first block together with the pseudo-words of the second half of the pairs and vice versa for the second block. Block order was counterbalanced across participants through their participant number (odd or even). Presentation order of the stimuli within a single block was randomized for each participant. The position of the stimuli was randomized, such that half of the stimuli appeared above the fixation cross and the other half below. Word – pseudo-word pairs were linked in the sense that they either appeared both above or below fixation. Furthermore, stimuli presented above and below fixation were matched in terms of word frequency (M_{above} = 2.33, M_{below} = 2.33, Bayes Factor = 6). Position was kept constant across participants. Prior to the start of the main experiment, participants completed 20 different practice trials to familiarize themselves with the task.

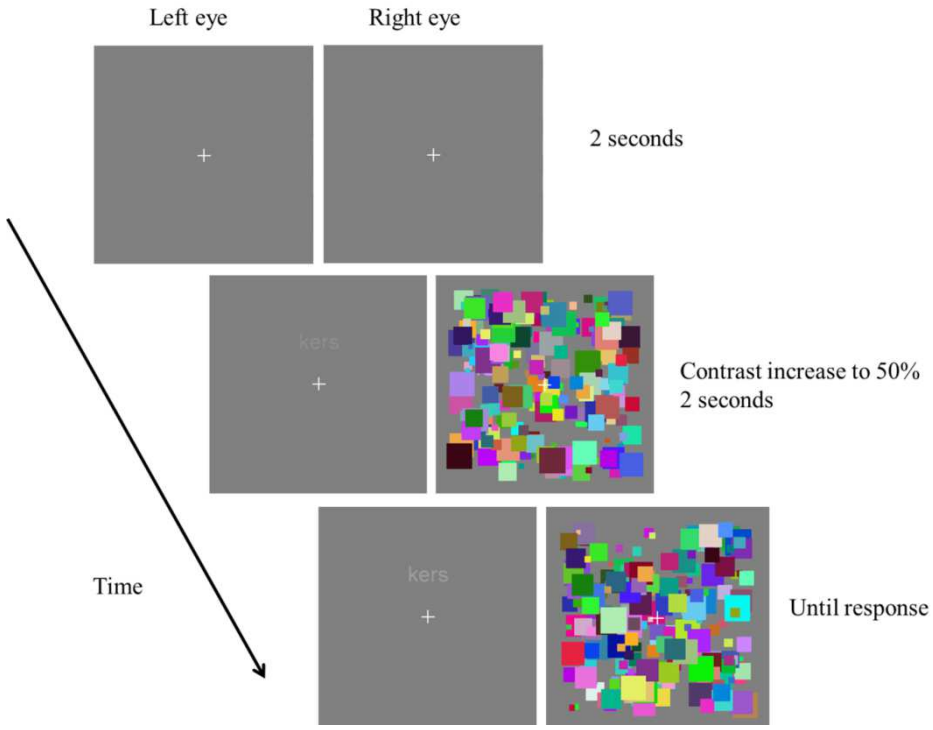


Figure 10.1. Example of the trial sequence. First, a fixation cross was presented for 2 seconds. Subsequently, the CFS mask was presented in one eye and the (non/pseudo)-word stimulus in the other. The (non/pseudo)-word stimulus increased in contrast from 0 to 50% over the course of 2 seconds and was continuously present until participants made a response.

Results and Discussion

All analyses were done on correct trials only (1.9% of the data had to be removed). Furthermore, data points below 500 ms or more than three standard deviations above each participant's mean suppression time were not included in the analysis (1.5% of the correct trials). Suppression times were log-transformed due to their positive skewness. All analyses were conducted within the Bayesian statistical framework using the BayesFactor package to calculate Bayes Factors (BF) and 95% credible intervals (Rouder & Morey, 2012; Rouder, Morey, Speckman, & Province, 2012). In contrast to classical null hypothesis testing, a Bayesian approach allows to quantify evidence in favor of either the null or the alternative hypothesis (Kruschke, 2011; Rouder & Morey, 2012; Rouder et al., 2012). All models tested here are so called mixed models as they consist of both fixed and random effects. The random part of the models was kept constant across all analyses and included random intercepts for participants and for words. To facilitate the interpretation of the results, we always z-transformed continuous variables and we also report t-statistics and 95%

confidence intervals for the same models using the lme4 package (Bates, Maechler, Bolker, & Walker, n.d., p. 4) (see Supplementary Table S10.1).

Figure 10.2 depicts average log suppression times for words and pseudo-words together with individual data points (left) and the relationship between word frequency and suppression time (right) (see Figure S10.1 for untransformed suppression times). Through eye balling the results it already becomes clear that there is neither an effect of word type nor word frequency on suppression time. This was confirmed in the BF analysis (see Table 10.2 for estimates of the fixed effects). The BF for a model including the effect of word type and random intercepts for participants and words was not favored over the random effects only model (BF = 26, i.e., the random effects only model was 26 times more likely). Similar results were obtained for the word frequency data (BF = 11). Both analyses were run separately since there was no meaningful value for word frequency of pseudo-words.

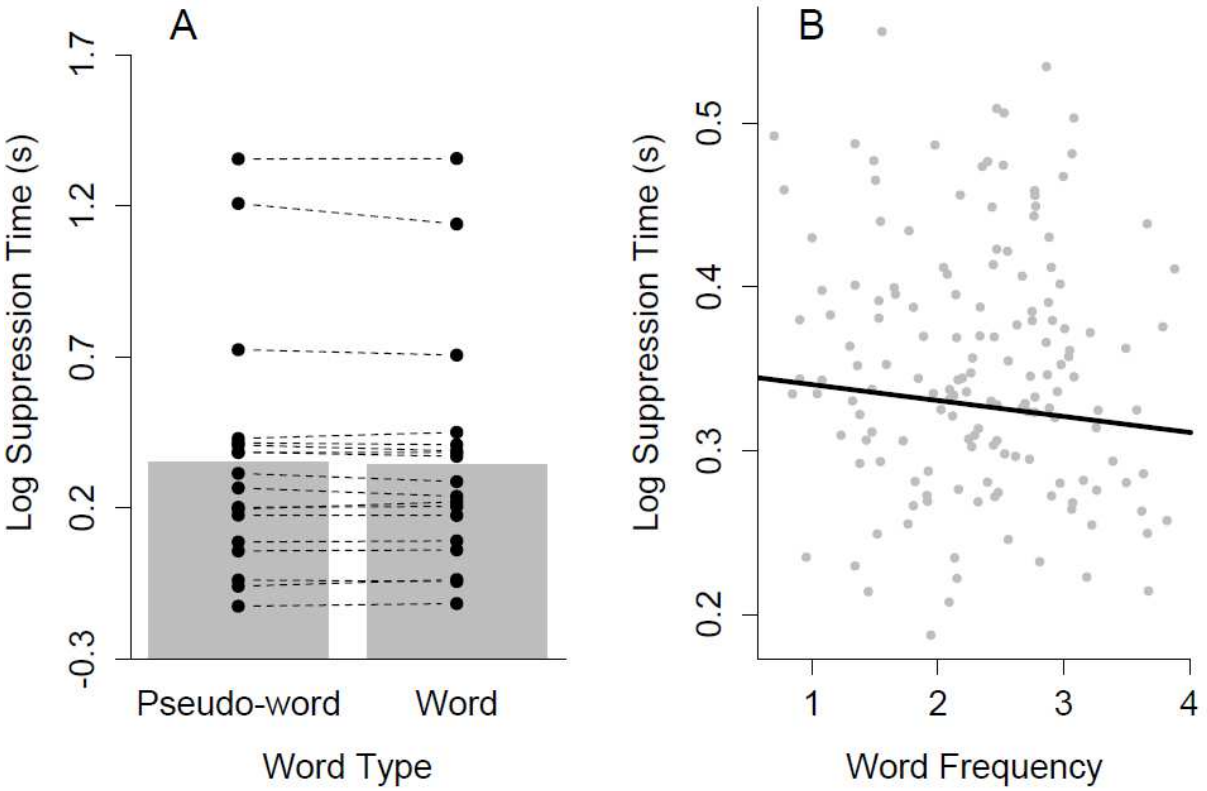


Figure 10.2. Results of Experiment 1. (A) The bar plot indicates mean log suppression times for words and pseudo-words. The dots show the mean log suppression time for each participant (connected dots refer to the same participant). (B) Scatter plot depicting the (absence of a) relationship between word frequency and log suppression time. The data points refer to mean log suppression time for each item averaged across participants. The

black line refers to the posterior estimate of the relationship between word frequency and log suppression time based on a mixed-effects model fit (with the BayesFactor package) with participants and words as crossed random effects and word frequency as a fixed effect.

Table 10.2. Point Estimates and 95% Credible Intervals of the Fixed Effects in Experiment 1 and 2.

Model	Experiment 1		Experiment 2	
	Estimate	95% CI	Estimate	95% CI
(1)				
Mu	0.354	[0.164 ; 0.557]	0.330	[0.226 ; 0.435]
Pseudo/Non - word	0.003	[-0.005 ; 0.010]	0.003	[-0.0006 ; 0.007]
Word	-0.003	[-0.010 ; 0.005]	-0.003	[-0.007 ; 0.0006]
Inverted	NA	NA	0.002	[-0.002 ; 0.006]
Upright	NA	NA	-0.002	[-0.006 ; 0.002]
(2)				
Mu	0.350	[0.160 ; 0.536]	0.330	[0.221 ; 0.435]
Word Frequency	-0.007	[-0.022 ; 0.007]	-0.007	[-0.020 ; 0.006]
Inverted	NA	NA	0.001	[-0.004 ; 0.007]
Upright	NA	NA	-0.001	[-0.007 ; 0.004]
(3)				
Mu	0.350	[0.161 ; 0.538]	0.330	[0.220 ; 0.434]
Word Frequency	-0.012	[-0.032 ; 0.009]	-0.003	[-0.020 ; 0.013]
Inverted	NA	NA	0.001	[-0.004 ; 0.007]
Upright	NA	NA	-0.001	[-0.007 ; 0.004]
Pixel Density	-0.016	[-0.030 ; -0.002]	-0.036	[-0.049 ; -0.025]
Trial	-0.053	[-0.064 ; -0.042]	-0.051	[-0.057 ; -0.046]

Age of Acquisition	-0.008	[-0.029 ; 0.011]	-0.004	[-0.019 ; 0.011]
Concreteness	-0.008	[-0.025 ; 0.009]	-0.005	[-0.018 ; 0.009]
(4)				
Mu	0.352	[0.162 ; 0.547]	0.332	[0.227 ; 0.437]
Pixel Density	-0.019	[-0.030 ; -0.009]	-0.035	[-0.046 ; -0.025]
Trial	-0.060	[-0.067 ; -0.052]	-0.052	[-0.056 ; -0.048]

Note. Per experiment, the parameter estimates of the fixed effects of four models are reported. Model (1) comprised only the main effect of word type (and of inversion in Experiment 2). Model (2) tested the main effect of word frequency (and inversion in Experiment 2). Model (3) is an expansion of Model (2) in that the main effects of pixel density, trial number, age of acquisition and concreteness were added. Finally, model (4) only consists of the main effects of pixel density and trial number. Models (1) and (4) were fitted using all data, model (2) was run on the word data only and model (3) included all words except one because concreteness and age of acquisition estimates were not available for this stimulus. To facilitate the comparison, all continuous variables were z-transformed (see Table 10.1 for means and standard deviations of the variables).

In addition, it is known that word frequency correlates with many other sublexical, lexical and semantic variables (Chalard, Bonin, Méot, Boyer, & Fayol, 2003; Morrison, Chappell, & Ellis, 1997). Hence, it is possible that the true word frequency effect was masked in the previous analysis. To test this hypothesis, a supplementary analysis was conducted in which a number of covariates were added to isolate the "pure" word frequency effect. That is, besides word frequency, we included main effects of age of acquisition, concreteness, pixel density and trial number. As age of acquisition and concreteness data were unavailable for one word, the analysis was performed on the remaining 153 words. Word length was left out to avoid potential multicollinearity issues as it correlated highly with pixel density (r = .80).

The estimates of the fixed effects (see Table 10.2) seem to suggest that neither concreteness, age of acquisition nor word frequency are related to suppression time as their corresponding 95% credible intervals all include zero. Trial number and pixel density on the

other hand, do seem to have an influence, in that suppression times became faster as the experiment advanced and as words contained more pixels. The obtained BFs confirm these findings (see Table 10.3). Two models are equally preferable, one with trial as only predictor and one with both trial and pixel density as predictors. All other models are at least eight times less likely.

Table 10.3. Bayes Factors for the Additional Analysis of Experiment 1.

Model	Bayes Factor
Trial	1
Pixel Density + Trial	1
Word Frequency + Pixel Density + Trial	8
Word Frequency + Trial	9
Pixel Density + Trial + Concreteness	10
Pixel Density + Trial + Age of Acquisition	10
Trial + Age of Acquisition	12
Trial + Concreteness	12
Word Frequency + Pixel Density + Trial + Concreteness	59
Word Frequency + Pixel Density + Trial + Age of Acquisition	62
Word Frequency + Trial + Concreteness	72
Word Frequency + Trial + Age of Acquisition	78
Pixel Density + Trial + Age of Acquisition + Concreteness	82
All other models	>100

Note. The Bayes Factor is relative to the model with trial number as only predictor and random intercepts for subjects and words. A Bayes Factor > 1 indicates evidence for the trial number only model. Models are ordered from low to high in terms of their Bayes Factor.

To further examine the effects of trial and pixel density, an additional analysis was run on both words and pseudo-words using only these two variables (see Table 10.2). The results are very similar, except that the model with both trial and pixel density was now clearly preferred over a trial only model (BF = 33), a pixel density only model (BF > 100) and a null model (BF > 100).

In Experiment 1, a set of word stimuli varying in word frequency and word type (word vs. pseudo-word) were presented under CFS and participants had to detect, upon breakthrough, as fast as possible whether the word stimulus was presented either above or below fixation. It was hypothesized that, given that semantic information of word stimuli is extracted in the absence of awareness, more frequent words would break suppression faster and words would break through suppression faster than pseudo-words. Contrary to our predictions, we found neither an effect of word frequency nor of word type. In additional analyses, we did however find a trial effect indicating that suppression times shortened over the course of the experiment (see (Ludwig, Sterzer, Kathmann, Franz, & Hesselmann, 2013) for similar observations). We interpret this trial effect as indicating that participants did not press randomly across the experiment, but were engaged in the task until the end. Moreover, pixel density of the stimuli also predicted suppression in that stimuli that comprised fewer pixels had longer response times. A similar effect was found by Lupyan and Ward (2013) using pictures as stimuli, which was taken to mean that the effectiveness of suppression depends on stimulus-driven factors like signal strength.

Although the evidence for a null effect in Experiment 1 was quite strong (according to the criteria advanced by Jeffreys (1961), alternative explanations can be devised as to why a null effect would be observed. First, the pseudo-words used in Experiment 1 were still word-like in the sense that they were pronounceable and orthographically similar to existing words. Thus, these pseudo-words might have activated the semantic network to an extent comparable to real words yielding no suppression time difference between words and pseudo-words. Therefore, in Experiment 2, non-words were generated by randomly jittering the individual letters of the words (e.g., *lamp* resulted in *mlap*). Second, one could argue that, although semantic information of words might not be processed, familiarity of the individual letters still is. Indeed, the potential role of stimulus familiarity (of the individual letter) cannot be disentangled from the design of Experiment 1. Therefore, we included a condition in Experiment 2 in which we presented the words and non-words inverted to assess the role of familiarity in breaking suppression (Gobbini, Gors, Halchenko, Rogers, et al., 2013; Stein, Sterzer, et al., 2012). Third, a potential criticism of Experiment 1 could be that our mask was just not sensitive enough to detect any difference between our conditions. It should be noted though, that this explanation is at odds with the observed pixel density effect. That is,

suppression appeared to be stronger when the bottom-up signal was relatively weak (see also Lupyan & Ward, 2013). Nevertheless, we addressed this in Experiment 2 by including a control experiment in which, instead of a word stimulus, a simpler stimulus (a white disc) was presented under CFS. The size of this disc was varied and it was hypothesized that the smaller disc would break suppression slower on average than the bigger disc, if suppression effectively takes place. Fourth, we observed that the consistency over participants in suppression time was rather low (i.e., Cronbach's $\alpha = .20$). Put differently, there was no stability across participants in which words broke suppression early and which words were relatively delayed. To further examine this issue, Experiment 2 consisted of a test-retest design such that it was possible to evaluate whether suppression time is stable *within* participants.

EXPERIMENT 2

Materials and Methods

Participants

Twenty new paid participants (4 male, age range 18 – 30 years) were recruited for Experiment 2. All participants had normal or corrected-to-normal vision and were naïve with respect to the goal of the study. Every participant provided informed consent before the start of the experiment. Note that, due to a programming error for participants with odd subject numbers, we had to rerun our original sample of 20 participants with 10 new participants with an odd subject number, but keeping the original participants with an even subject number. Furthermore, 4 participants were not included because they did not complete the full experiment. One of them did not show up for the retest session, the others did not finish the first session due to suppression being too effective.

Apparatus

The experimental set-up was the same as in Experiment 1.

Stimuli

All stimuli were the same as in Experiment 1 except for the following. A partially new set of word stimuli was created to ensure that the findings from Experiment 1 could not be

attributed to the specific stimulus set used (see Table 10.1 for a summary of the stimulus characteristics). Word length varied from three to seven letters and word frequency from 0.669 to 3.882. There were 115 words in total, from which unpronounceable non-words were created by shuffling the letters. To test the effect of stimulus familiarity, the 230 words and non-words were inverted, thus yielding 460 stimuli in total. The size of the words ranged from 1.56° to 4.35° depending on the length of the word. The height of the words was maximally 0.92°.

In the control experiment, a white disc was presented as a target instead of a word. The radius of the disc was manipulated to be either 0.6° or 1.2°.

Procedure

The experimental procedure was similar to Experiment 1. Prior to the start of the main experiment, participants completed the eye dominance experiment. The task in the main experiment was exactly the same as in Experiment 1. In the control experiment, a white disc increasing in contrast from 0 to 100% over the course of 2 seconds was presented either 2° above or below fixation. As in the main experiment, participant had to indicate the location of the disc as fast as possible once it broke suppression through a button press on the numerical keyboard (1 for above, 3 for below). A second session always took place 24 hours after the first session and included only the main and control experiment.

Design

The main independent variables were word type (word vs. non-word), inversion (upright vs. inverted) and word frequency (ranging from 0.669 to 3.882). In the control experiment, disc radius was manipulated (small vs. large; 0.6° vs. 1.2°). Before the start of the main experiment, participants again performed 20 practice trials on a different set of stimuli. The main experiment now consisted of 460 trials (i.e., 115 words, 115 non-words and their inverted counterparts) and therefore was split up into four blocks. Similar to Experiment 1, the words of a random half of the word – non-word pairs were presented in the first half of the experiment together with the non-words of the second half of the pairs and vice versa. The position of the stimuli was again determined at random and kept constant across participants. Similar to Experiment 1, word – non-word pairs and their inverted counterparts

were all either presented above or below fixation. As a result the number of stimuli appearing above and below fixation was not perfectly identical (i.e., 232 stimuli below fixation and 228 above). Stimuli were again matched on word frequency (M_{above} = 2.24, M_{below} = 2.34, BF = 4).

After completing the main experiment, the experimenter started the control experiment in which participants had to detect a white disc that was either presented 2° above or below fixation. They first completed 20 practice trials and subsequently performed 100 trials in the control experiment (50 per condition, randomized on each trial). On the second day, participants returned to perform the experiments in the same order again, except for the eye dominance measurement which was not repeated.

Results and Discussion

Main experiment.

Figure 10.3 summarizes the results of Experiment 2 (see Figure S10.2 for untransformed suppression times). As in Experiment 1, analyses were done on the logarithmically transformed suppression times after removal of inaccurate responses (1.6% of all data) and outlying data points (defined as being below 500 ms or higher than each participant's mean suppression times plus three times the standard deviation; 1.5% of all correct trials). Again, all models fitted here are mixed models with random intercepts for participants and for words.

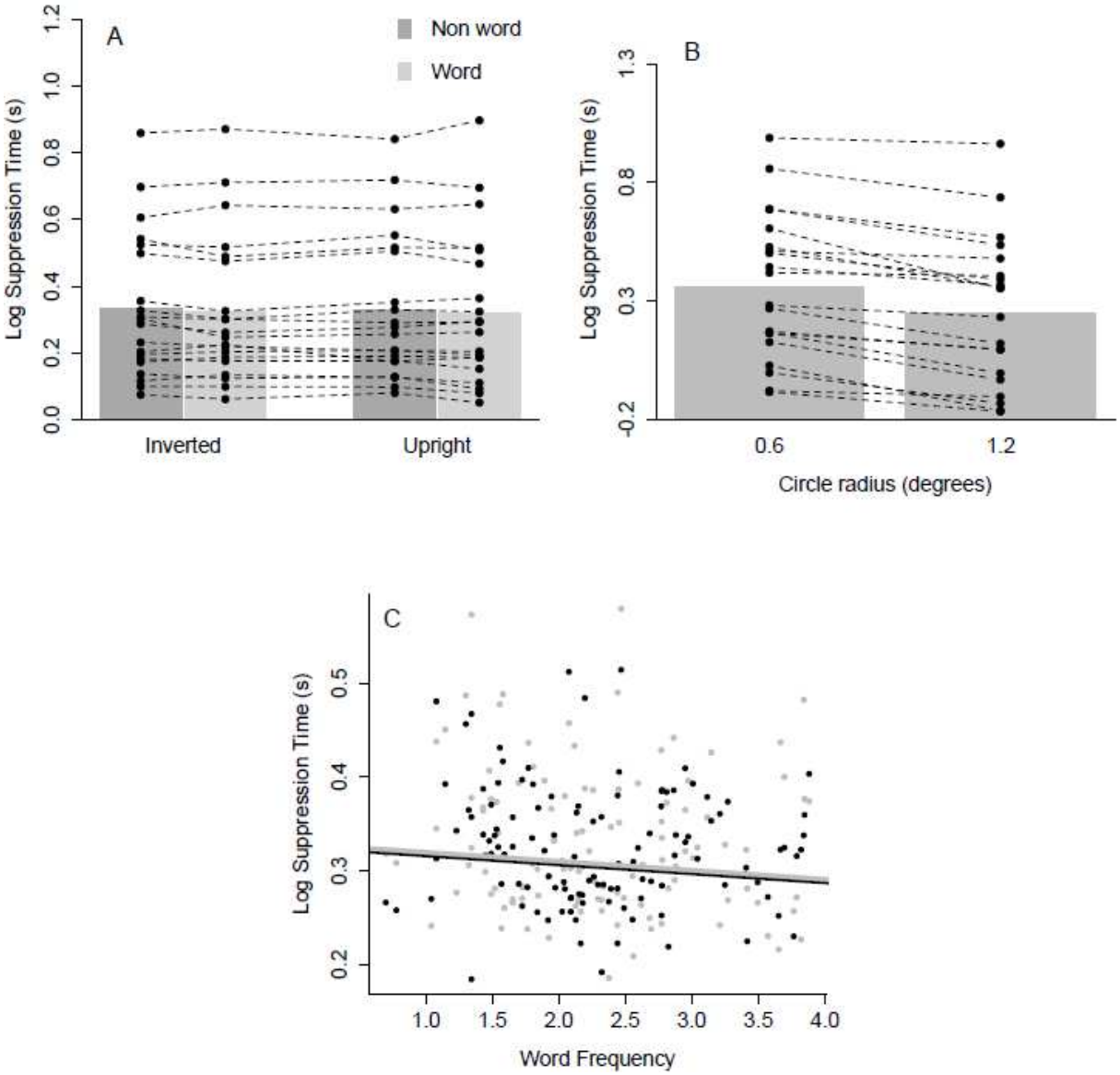


Figure 10.3. Results of Experiment 2. (A) The bar plot depicts the mean log suppression times for each condition. The dots refer to mean log suppression times per participant (connected dots refer to the same participant). (B) Mean log suppression times for the control experiment. The bar plot depicts the grand mean for both conditions whereas the dots refer to single participants (connected dots refer to the same participant). (C) Scatter plot depicting the (absence of a) relationship between word frequency and log suppression times for upright (black) and inverted (gray) words. The black and gray lines (hardly discernible) refer to the estimates of the relationship between word frequency and log suppression time after a mixed-effects model fit with subject and word as crossed random effects and word frequency and inversion as fixed effects.

The results shown in Table 10.4 indicate that neither an effect of word type nor inversion nor an interaction between both is present in the data (see also Table 10.2 for the parameter estimates of the model including only the main effects of word type and inversion). In addition, an analysis on only the word stimuli did not reveal an effect of word frequency, inversion or an interaction between both variables (see Table 10.5 for Bayes Factors; and Table 10.2 for parameter estimates of the main effects only model). Taken together, the (empty) random intercepts only model was always preferred.

Table 10.4. Bayes Factors for the Analysis of Word Type and Inversion of Experiment 2.

Model	**Bayes Factor**
Random Intercepts Only	1
Word Type	15
Inversion	45
Word Type + Inversion	> 100
Word Type * Inversion	> 100

Note. The Bayes Factor is relative to the null model, including only random intercepts for subjects and words. A Bayes Factor > 1 indicates evidence for the null model.

Table 10.5. Bayes Factors for the Analysis of Word Frequency and Inversion of Experiment 2.

Model	**Bayes Factor**
Random Intercepts Only	1
Word Frequency	10
Inversion	36
Word Frequency + Inversion	> 100
Word Frequency * Inversion	> 100

Note. The Bayes Factor is relative to the null model, including only random intercepts for subjects and words. A Bayes Factor > 1 indicates evidence for the null model.

As in Experiment 1, we ran an additional analysis to statistically control for confounding variables (i.e., concreteness, age of acquisition, pixel density and trial number). The results replicate our previous findings in that the model with trial number and pixel

density was preferred over all other models by a factor of at least ten (see Table 10.6). Also, when looking at the model with the main effects of word frequency, inversion, concreteness, age of acquisition, pixel density and trial number, it can be seen that only the 95% credible intervals of trial and pixel density exclude zero (see Table 10.2). The effects of trial and pixel density were confirmed in an additional analysis on both words and non-words using only these two predictors (see Table 10.2). That is, the model with both trial and pixel density was the best fitting model (all BFs > 100).

Table 10.6
Bayes Factors for the Additional Analysis of Experiment 2.

Model	Bayes Factor
Pixel Density + Trial	1
Pixel Density + Trial + Concreteness	10
Pixel Density + Trial + Age of Acquisition	12
Word Frequency + Pixel Density + Trial	12
Word Inversion + Pixel Density + Trial	36
Pixel Density + Trial + Age of Acquisition + Concreteness	97
All other models	>100

Note. The Bayes Factor is relative to the model with trial number and pixel density as predictors and random intercepts for subjects and words. A Bayes Factor > 1 indicates evidence for the trial number and pixel density only model. Models are ordered from low to high in terms of their Bayes Factor.

Control experiment.

As is apparent from Figure 10.3, the data from the control experiment indicate an effect in the predicted direction. Concretely, the large disc broke through suppression faster than the small disc. This was confirmed by a Bayes factor (BF > 100). The model including circle radius as a fixed effect and random subject intercepts was preferred over the random intercepts only model. The null effects observed in the main experiment can therefore not be attributed to a general lack of suppression elicited by our CFS mask.

Test-retest reliability.

Figure 10.4 depicts a histogram of the test-retest reliability scores for each participant in the main experiment. These correlations were computed by correlating the log suppression times for all 460 stimuli obtained in session 1 with those obtained in session 2. The mean test-retest reliability score was equal to .16 (ranging from -.10 to .37). Note that recalculating the test-retest reliability for the word stimuli only did not improve these correlations (mean .16, range from -.12 to .38). The right panel of Figure 10.4 depicts the test-retest reliability for the control experiment. Since this experiment only included repetitions of the same two stimuli, the effect size (Cohen's d) for circle radius was computed for each participant on each session and correlated between sessions, yielding a correlation of .51.

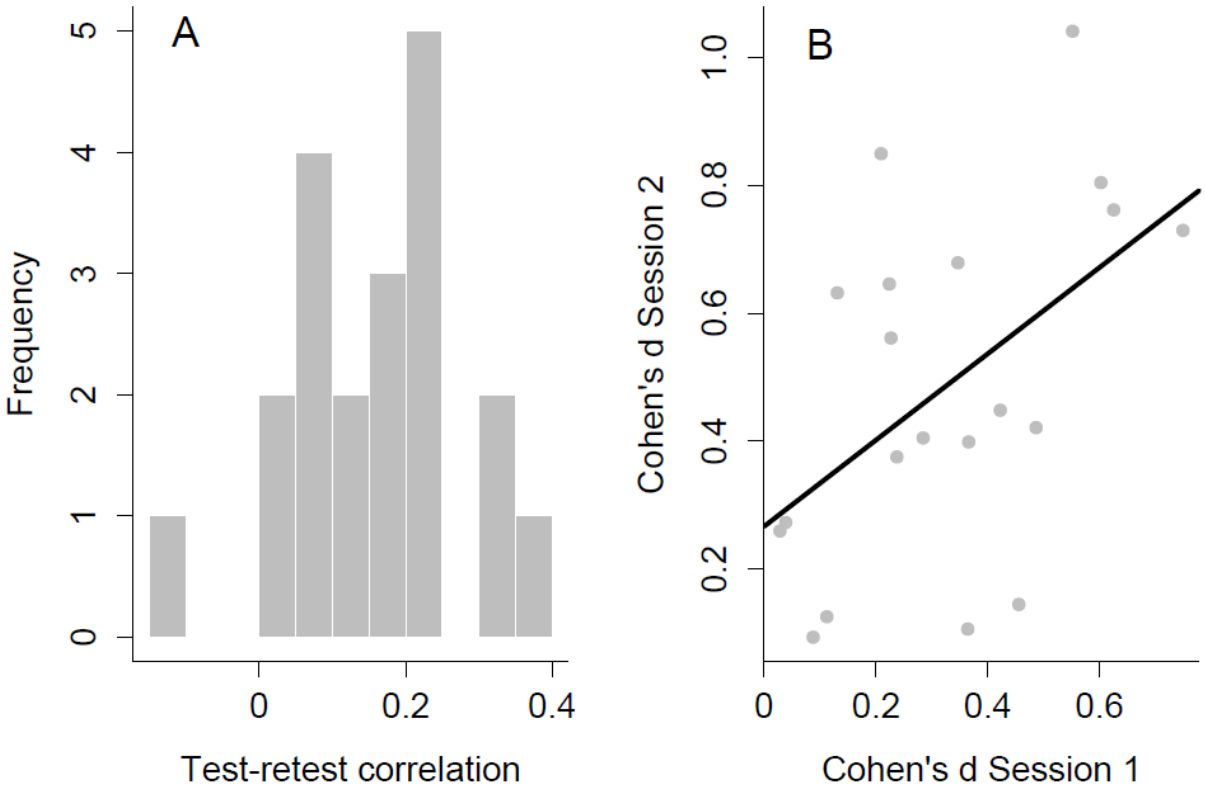


Figure 10.4. Test-retest reliability. (A) Histogram of test-retest correlations for every participant. (B) Scatterplot between the effect sizes obtained in sessions 1 and 2 of the control experiment. The black line refers to the best fitting regression line obtained from a simple linear regression of session 2 effect sizes on session 1 effects sizes.

In Experiment 2, some potential alternative explanations for the absence of a suppression time difference between words and pseudo-words were explored. First, the

pseudo-words used in Experiment 1 might still have elicited some partial semantic activation, obscuring an effect of unconscious processing of semantic information. Therefore, in Experiment 2 unpronounceable non-words were used, generated by scrambling the individual letters of each word stimulus. Still, no evidence for an effect of word type was obtained. Secondly, stimulus familiarity might have contributed to the suppression times for each condition in Experiment 1 instead of semantic processing. Therefore, we included a condition in which the words and non-words were inverted, to examine the effect of stimulus familiarity while keeping low-level characteristics of the stimulus constant. Surprisingly, no evidence of an inversion effect was obtained, contrary to previous findings (Yang & Yeh, 2011, 2014). In hindsight, the absence of an inversion effect is not that surprising given that inverting letters in the Latin alphabet does not always have a disruptive effect. That is, five letters remain the same when inverted (i.e., l, o, s, x, and z), six become another letter (i.e., b, d, n, p, q, and u), and some remain letter-like (e.g., m and w).

Third, the results of Experiment 1 showed low consistency across participants in suppression times. Therefore, Experiment 2 employed a test-retest design to further probe the reliability in both the main and control experiment. Test-retest reliability in the main experiment was on average rather low, indicating that there is considerable instability in the suppression times *within* participants. In the control experiment, the test-retest correlation approximated the estimate reported in Yang et al. (2010) in which a similar measure was correlated across sessions. Although the latter correlation was still far from perfect, its comparability with the correlation reported in Yang et al. (2010) speculatively hints at a potential ceiling for correlations of effect sizes based on stimulus manipulations in the CFS paradigm. Note that this does not mean that the data from the main experiment have no structure whatsoever. That is, some subjects showed a position bias for stimuli either presented above or below fixation and these effects correlated well across sessions (test-retest correlation for Cohen's d of the position effect in the main experiment was .87).

Finally, the results of Experiment 1 could have potentially been explained by a lack of suppression initiated by the CFS masks or by a general insensitivity to detect any effect. To address this issue, a control experiment was conducted in which a simple stimulus, a white disc, was varied in radius. It was predicted that a large disc would break suppression faster than a small disc and the results of the control experiment confirmed this prediction.

However, both the radius effect in circles and the pixel density effect in letter strings are fairly low-level. In principle it is possible that the present set-up is merely not sensitive enough to capture any high-level effect. That is, the lack of a word inversion effect could indicate a general lack of obtaining inversion effects using our implementation of b-CFS. To address this issue we set out to replicate the widely reported face inversion effect, in which faces presented upright break suppression faster than inverted faces (Gobbini, Gors, Halchenko, Rogers, et al., 2013; Jiang et al., 2007; Stein, Hebart, et al., 2011; Stein, Peelen, et al., 2011; Stein, Senju, et al., 2011; Stein, Seymour, et al., 2014; Stein, Sterzer, et al., 2012; E. Yang et al., 2007; G. Zhou et al., 2010). In Experiment 3 the same b-CFS set-up was used, but the suppressed stimuli were (inverted or upright) faces instead of letter strings. If our b-CFS design is indeed unable to obtain high-level effects, one would expect no face inversion effect. Alternatively, finding a robust face inversion effect in light of the results of Experiments 1 and 2, would suggest that word frequency, word type and letter inversion have genuinely no effect on suppression times.

EXPERIMENT 3

Materials and Methods

Participants

Eight volunteers participated in the experiment (3 male, age range 24 – 34 years). All had normal or corrected-to-normal vision and were naïve with respect to the goal of the study. Every participant provided informed consent before the start of the experiment.

Apparatus

The experimental set-up was the same as in Experiments 1 and 2.

Stimuli

The same CFS mask was used as in Experiments 1 and 2. The face stimuli were obtained from the NimStim database (Tottenham et al., 2009). Ten neutral faces were picked from the database (five male). These were resized to approximately 2.1° x 2.6° (similar to Stein et al. (Stein, Hebart, et al., 2011)). Four different neutral faces (two male) were used for the practice trials.

Procedure

The experimental procedure was similar to Experiments 1 and 2. A trial started with a 2 second presentation of the fixation cross after which the CFS mask was presented in the dominant eye and the upright or inverted face stimulus in the non-dominant eye. As in Stein et al. (Stein, Hebart, et al., 2011), the face stimulus was presented at a random location to the left or right of fixation. The participants were instructed to report as quickly as possible the location of the stimulus (left or right relative to fixation, through a button press) upon the moment it broke suppression.

Design

The only independent variable was inversion (upright vs. inverted). Prior to the start of the main experiment, participants completed 16 practice trials to familiarize themselves with the task. During the main experiment, participants completed 120 trials in three blocks of 40 trials. For each participant, all ten faces were presented equally often in the inverted as in the upright condition and they were shown right of fixation in half of the trials and left in the other half. The order of the trials was randomized.

Results and Discussion

As in Experiments 1 and 2, all reported analyses were done on the logarithmically transformed response times after removal of inaccurate (1.5 %) and outlying data points (defined as below 500 ms or higher than each participant's mean suppression time plus three times the standard deviation; 1.6 % of all correct trials). Figure 10.5 summarizes the results of Experiment 3 (see Figure S10.3 for untransformed suppression times). There appears to be a strong inversion effect in that upright faces break through suppression faster than inverted faces. This was confirmed by comparing the model with face inversion as a factor against an empty model (both models also included random intercepts for participants and for faces). Specifically, the Bayes Factor indicated a clear preference for the model including face orientation over the empty model (BF > 100). Furthermore, the 95% credible interval did not include zero (95% CI: [0.09; 0.18]).

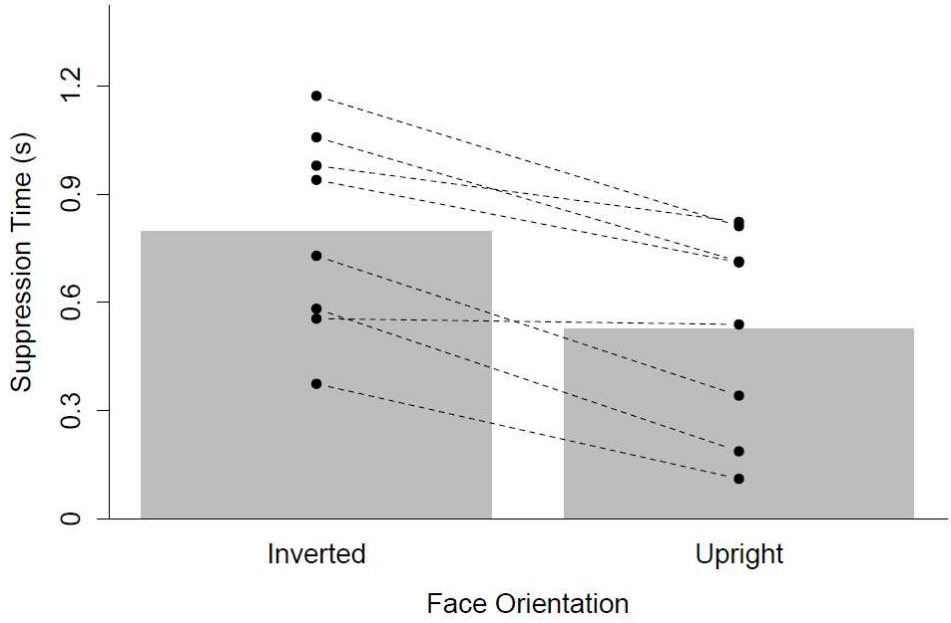


Figure 10.5. Results of Experiment 3. The bar plot indicates mean log suppression times for upright and inverted faces. The dots show the mean log suppression time for each participant (connected dots refer to the same participant).

Experiment 3 clearly replicated the face inversion effect, one of the most robust findings in the b-CFS literature, using the same set-up as in Experiments 1 and 2. Indeed, the absence of an inversion effect obtained in Experiment 2 could be due to a general lack of obtaining any kind of familiarity effect using our design (in contrast to our explanation of inverting individual letters of the Latin alphabet not effectively disrupting familiarity). The present results rule out the possibility that our b-CFS implementation disrupted any sensitivity to find inversion effects. Thus, it is not the case that the employed set-up did not allow us to detect high-level effects. This suggests that the findings of Experiments 1 and 2 genuinely reflect that more high level characteristics such as word frequency, word type (words vs. non-words) and letter inversion do not influence suppression times.

GENERAL DISCUSSION

The goal of this study was to explore two hypotheses regarding unconscious processing of semantic information of words presented under CFS. First, it was predicted that existing words would break suppression faster than their pseudo-word/non-word variants. Second, we tested whether the suppression time of words is modulated by their

frequency, resembling the word frequency effect in visual word recognition. Across two experiments, we found neither a word type effect nor a frequency effect. While the lack of a word type effect in Experiment 1 could be attributed to the use of pronounceable pseudo-words as a baseline, Experiment 2 excluded this explanation, as words did not break suppression faster than unpronounceable non-words. In addition, the fact that there was a consistent negative relation between pixel density and suppression time, suggests that the observed null results can not be attributed to the paradigm being insensitive to differences in detectability. The latter was further supported by Experiment 3, which showed that upright faces broke suppression faster than inverted ones using the exact same b-CFS set-up. Thus, even though the employed paradigm can capture high-level effects, only variability in low-level word characteristics like pixel density led to differential suppression times. Taken together, our findings do not support the claim that words are processed up to a semantic level under CFS.

In the visual masking literature on the other hand, unconscious semantic processing has been established (Marcel, 1983; Van den Bussche, Van den Noortgate, & Reynvoet, 2009). Should there be any reason to expect differences between visual masking and CFS paradigms? Discrepancies between unconscious processing of emotional information of faces have been reported in the context of masking, interocular suppression, and gaze-contingent crowding (Faivre, Berthet, & Kouider, 2012). In visual masking, one explanation as to why the masked stimulus does not enter visual awareness is that re-entrant activation from higher cortical areas, presumably associated with perceiving the stimulus (Lamme & Spekreijse, 2000; Lamme, Supèr, Landman, Roelfsema, & Spekreijse, 2000), is nearly absent, yet the feed-forward sweep of activation associated with presentation of the masked stimulus is largely intact (Breitmeyer, 2008; Macknik & Livingstone, 1998). CFS, however, relies on binocular rivalry of which the suppression mechanisms have mostly been attributed to inhibition between monocular neurons, although most recent models of binocular rivalry indicate potential inhibition mechanisms between higher levels of the visual system also (Blake, 1989; Blake & Logothetis, 2002; Tong et al., 2006). Indeed, neuroimaging studies have indicated that processing of suppressed stimuli beyond striate areas is largely absent along the ventral visual pathway (Fang & He, 2005; Hesselmann & Malach, 2011). As a consequence, any processing of semantic information under CFS seems implausible. Indeed,

in a standard dissociation study using CFS, Kang, Blake, and Woodman (2011) explicitly showed that parametrically manipulating target visibility attenuated the amplitude of the N400 component (an index of semantic congruency) until it was absent when observers could not discriminate the meaning of the suppressed words.

An alternative explanation of the present findings is that semantic information is indeed extracted under CFS, but that the b-CFS paradigm is ill-suited to unambiguously detect these effects. That is, our results showed that suppression times are unstable both between and within participants. Such a poor reliability has rather dramatic effects on the probability of detecting a true underlying relation. An average test-retest reliability of .16 as observed in Experiment 2 could attenuate a true correlation of, say .60, to .24 (note that this example only considers the reliability of one variable, in this case suppression time, thereby (unrealistically) assuming that the other measure (e.g., word frequency) is perfectly reliable. In practice, the .24 estimate may thus even prove to be too optimistic). So even if there actually is a relation between suppression breaking and word frequency, it might go undetected using this paradigm. In comparison, reliability estimates of (log-transformed) response times in traditional word recognition studies generally range from .70 to .90 (e.g., Keuleers, Diependaele, & Brysbaert, 2010; Keuleers et al., 2012). However, the low reliability observed here is specific to our stimuli and does not need to generalize to other stimuli like pictures or the b-CFS paradigm in general.

Furthermore, it should be noted that criticisms have been raised concerning the validity of the b-CFS paradigm to infer unconscious processing of suppressed stimuli (Stein, Hebart, et al., 2011; Stein & Sterzer, 2014). That is, the dependent measure used in b-CFS studies is the time it takes for subjects to be able to make a response on a certain attribute of the suppressed stimulus (e.g. its location). This suppression time measure per se is based on *conscious* processing. However, the argument to use b-CFS as a valid way to infer unconscious processing is that differences in suppression times are attributable to unconscious processing of the stimulus while suppressed. For this reasoning to be valid, the observed suppression time differences should be due to CFS-specific processing and not non CFS-specific threshold differences. To rule out this possibility, Jiang et al. (2007) and subsequent studies usually implemented a binocular control condition in which the CFS mask and stimulus are simultaneously presented in both eyes. However, Stein, Hebart, et al.

(2011) have recently shown that this control condition is ill-suited to exclude non CFS-specific processing in the CFS condition since both conditions differ on aspects other than CFS-specific processing. Based on these findings, Stein and Sterzer (2014) recently argued that b-CFS, as it is currently implemented, can not unequivocally provide evidence for unconscious processing of the suppressed stimulus.

Taken together, the criticisms raised by Stein and colleagues (Stein, Hebart, et al., 2011; Stein & Sterzer, 2014) and our low reliability estimates seem to imply that the use of b-CFS as a paradigm to study unconscious semantic processing of words is questionable. Hence, we would argue that other paradigms combined with CFS might be more appropriate to probe the nature of processing of suppressed words (see also Heyman & Moors, 2012). For example, it might be valuable to present suppressed words as primes and to study their influence on the reaction times to (un)related targets in, for example, a lexical decision task. Nevertheless, the question remains as to which mechanisms underlie the (seemingly contradicting) effects observed in the literature. Below, we offer some speculative explanations, but it should be noted that future research and/or re-analysis of existing datasets is needed to assess their validity.

One possibility is that familiar stimuli break suppression faster than unfamiliar stimuli (Gobbini, Gors, Halchenko, Rogers, et al., 2013; Stein, Sterzer, et al., 2012). Such a familiarity effect has been observed by Jiang et al. (2007). In one of their experiments, Chinese and Hebrew speakers were presented with Chinese and Hebrew words under CFS. Jiang et al. (2007) observed that Chinese words broke suppression faster for Chinese speakers as well as Hebrew words for Hebrew speakers. Furthermore, Yang and Yeh (2011) also examined familiarity effects by comparing upright words with inverted and phase-scrambled words. Both inverting and phase-scrambling the character words significantly increased suppression times relative to upright words. These findings together with those of Jiang et al. (2007) do provide evidence for a potential familiarity effect under b-CFS. In contrast, we did not obtain an inversion effect in Experiment 2, but in hindsight this is not entirely unexpected if individual characters are the locus of the familiarity effect. Specifically, inverting Latin letters often yields the same letter (e.g., *o*) or a different letter (e.g., *d* becomes *p* and vice versa), thus yielding (partially) familiar character strings. In addition, research

shows that an inversion effect is not ubiquitous. For instance, Stein, Sterzer, et al. (2012) found an inversion effect of human faces and bodies, but not of inanimate objects.

Note that a familiarity effect could be the result of bottom-up processes (i.e., unconscious processing occurs to a certain extent under CFS and familiar stimuli, or familiar parts, break suppression faster) as well as top-down processes (i.e., subjects generate familiar representations that are matched with the visual input, which in turn facilitates suppression breaking). The latter mechanism could also explain the priming effect found by Costello et al. (2009). Presumably, subjects generate a set of candidate targets based on the prime (e.g., *dog*, *pet*, *animal* when the prime is *cat*). The visual representation of these candidates might boost the detection of the actual target, when prime and target are indeed related, through a matching process. In a recent study by Lupyan and Ward (2013), a similar biasing effect has been reported in that informative verbal labels (presented auditorily) biased detection performance of suppressed visual stimuli relative to uninformative verbal labels. This effect was attributed to top-down activation of the visual shape properties of the suppressed stimuli which eventually biased the competition process (Lupyan & Ward, 2013). Note that such a top-down process implies that, for example in the study of Costello et al. (2009), semantic processing of the suppressed stimulus does not necessarily have to occur. That is, the prime stimulus could activate visual representations of related words acting as a predictive signal for the visual system (see Lupyan & Ward, 2013).

Another explanation is based on the data-analysis method used in many studies. That is, most studies only perform a standard repeated measures ANOVA on (log-transformed) suppression times averaged across stimuli (i.e., the so-called F_1 test). In psycholinguistics, this has been referred to as the "language-as-fixed-effect" fallacy (Clark, 1973) and incorporating stimulus as a random effect is standard in psycholinguistics nowadays. The importance of this practice has recently been demonstrated by Barr, Levy, Scheepers, and Tily (2013) in a simulation study. In short, they showed that performing only an F_1 test dramatically increases Type 1 errors especially for between-item manipulations (in that respect, it is interesting to remark that a classical repeated measures F_1 analysis on the data of the main experiment of Experiment 2 yielded a marginally significant effect of word type ($F_1(1,19) = 3.3$, $p = .09$)). Furthermore, in order to quantify the evidence in favor of one or the other model, statistical inference in this study was done in a Bayesian framework, which has

shown to be more conservative than traditional null hypothesis significance testing with respect to the strength of the evidence for an effect (Wetzels et al., 2011). This allows one to quantify evidence in favor of the hypothesis that no semantic processing occurs under CFS, while traditional test cannot confirm the null hypothesis (Kruschke, 2011; Wagenmakers, 2007).

CONCLUSION

In this study, the extent to which words are semantically processed in the absence of awareness (induced by CFS) was studied. In Experiment 1, no evidence was obtained for differential processing between word and pseudo-word stimuli nor a modulation of suppression time of words by word frequency. In Experiment 2, the absence of these effects was replicated. In contrast, a control experiment with a simpler stimulus showed that large white discs break suppression faster than small white discs. Finally, Experiment 3 replicated the face inversion effect, thus ruling out the possibility that the null effects were merely caused by our experimental set-up being insensitive to any high-level manipulation. These results were explained from the perspective that the suppressed stimuli might not have been processed up to the level at which semantic information is usually extracted. Alternatively, due to the instability of suppression times within and between participants, b-CFS might be an ill-suited paradigm to study unconscious semantic processing of words.

SUPPLEMENTARY FIGURES

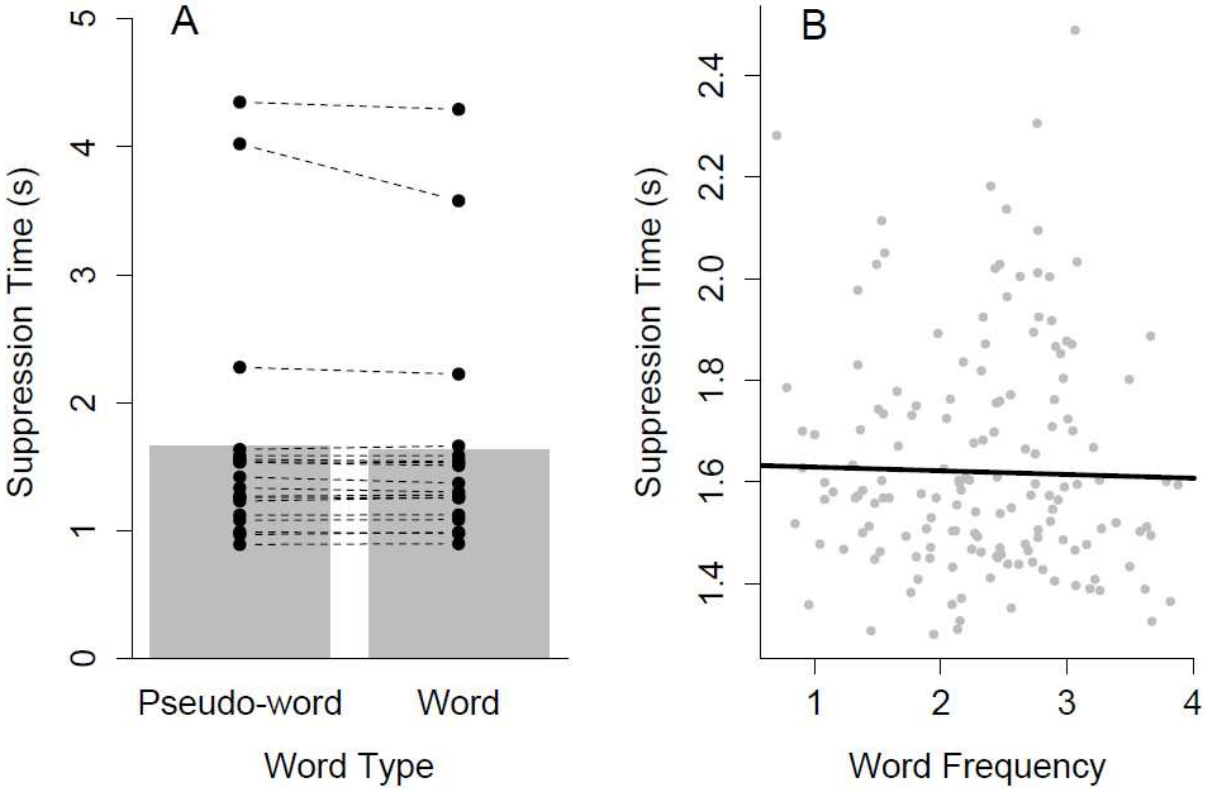


Figure S10.1. Results of Experiment 1. (A) The bar plot indicates mean suppression times for words and pseudo-words. The dots show the mean suppression time for each participant (connected dots refer to the same participant). (B) Scatter plot depicting the (absence of a) relationship between word frequency and suppression time. The data points refer to mean suppression time for each item averaged across participants. The black line refers to the posterior estimate of the relationship between word frequency and suppression time based on a mixed-effects model fit (with the BayesFactor package) with participants and words as crossed random effects and word frequency as a fixed effect.

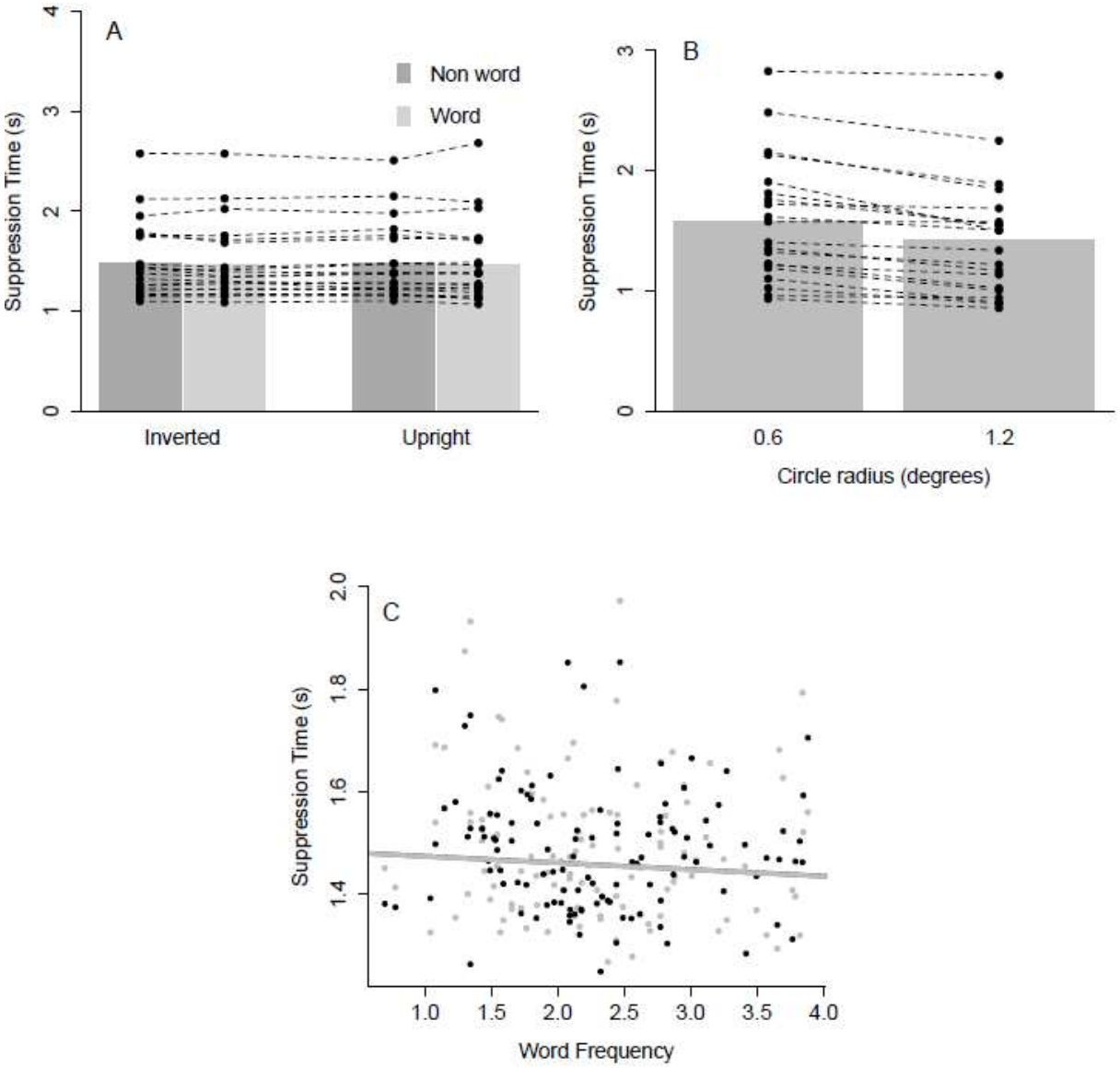


Figure S10.2. Results of Experiment 2. (A) The bar plot depicts the mean suppression times for each condition. The dots refer to mean suppression times per participant (connected dots refer to the same participant). (B) Mean suppression times for the control experiment. The bar plot depicts the grand mean for both conditions whereas the dots refer to single participants (connected dots refer to the same participant). (C) Scatter plot depicting the (absence of a) relationship between word frequency and suppression times for upright (black) and inverted (gray) words. The black and gray lines (hardly discernible) refer to the estimates of the relationship between word frequency and suppression time after a mixed-effects model fit with subject and word as crossed random effects and word frequency and inversion as fixed effects.

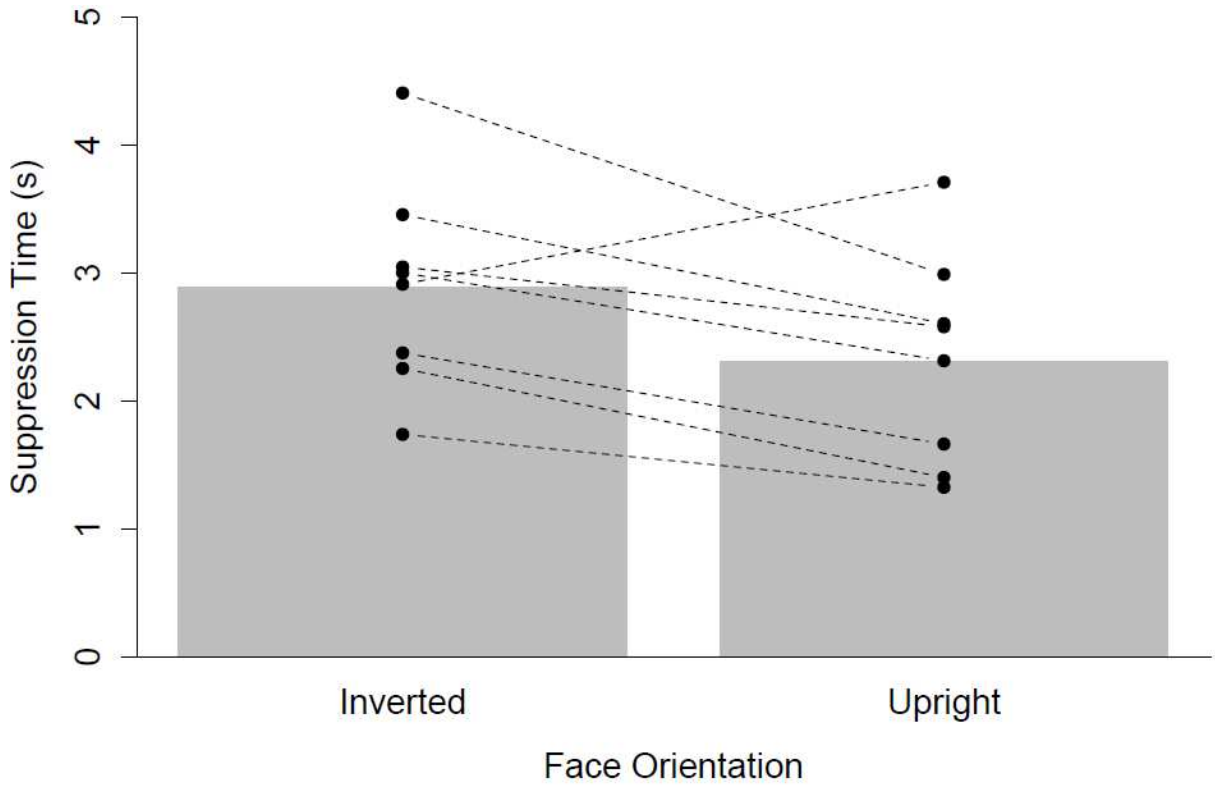


Figure S10.3. Results of Experiment 3. The bar plot indicates mean suppression times for upright and inverted faces. The dots show the mean suppression time for each participant (connected dots refer to the same participant).

SUPPLEMENTARY TABLE

Table S10.1

Point Estimates, t-values and 95% Confidence Intervals of the Fixed Effects in Experiment 1 and 2.

Model	Experiment 1			Experiment 2		
	Estimate	t	95% CI	Estimate	t	95% CI
(1)						
Mu	0.353	3.818	[0.167 ; 0.538]	0.331	6.651	[0.231 ; 0.431]
Pseudo/Non - word	0.003	0.696	[-0.005 ; 0.011]	0.003	1.653	[-0.0006 ; 0.007]
Word	-0.003	-0.696	[-0.011 ; 0.005]	-0.003	-1.653	[-0.007 ; 0.0006]
Inverted	NA	NA	NA	0.002	0.765	[-0.002 ; 0.006]
Upright	NA	NA	NA	-0.002	-0.765	[-0.006 ; 0.002]

(2)						
Mu	0.350	3.841	[0.167 ; 0.541]	0.328	6.495	[0.227 ; 0.429]
Word Frequency	-0.007	-1.209	[-0.019 ; 0.005]	-0.007	-1.227	[-0.019 ; 0.004]
Inverted	NA	NA	NA	0.001	0.499	[-0.004 ; 0.007]
Upright	NA	NA	NA	-0.001	-0.499	[-0.007 ; 0.004]
(3)						
Mu	0.352	3.847	[0.168 ; 0.536]	0.329	6.542	[0.228 ; 0.430]
Word Frequency	-0.012	-1.416	[-0.028 ; 0.004]	-0.003	-0.448	[-0.017 ; 0.011]
Inverted	NA	NA	NA	0.001	0.518	[-0.004 ; 0.007]
Upright	NA	NA	NA	-0.001	-0.518	[-0.007 ; 0.004]
Pixel Density	-0.016	-2.728	[-0.028 ; -0.005]	-0.037	-7.045	[-0.047 ; -0.027]
Trial	-0.053	-9.714	[-0.064 ; -0.042]	-0.051	-18.120	[-0.057 ; -0.046]
Age of Acquisition	-0.008	-1.001	[-0.025 ; 0.008]	-0.004	-0.577	[-0.017 ; 0.009]
Concreteness	-0.008	-1.062	[-0.022 ; 0.006]	-0.005	-0.876	[-0.017 ; 0.006]
(4)						
Mu	0.354	3.822	[0.168 ; 0.540]	0.332	6.674	[0.232 ; 0.431]
Pixel Density	-0.018	-4.142	[-0.027 ; -0.010]	-0.036	-7.373	[-0.045 ; -0.026]
Trial	-0.060	-15.079	[-0.067 ; -0.052]	-0.052	-25.996	[-0.056 ; -0.048]

Note. See Table 10.2 for an explanation of the models. One can consider the effect of a variable significant (i.e., $p < .05$, two-tailed) if the absolute value of the t-statistic is above 1.96.

However, as Barr et al. (2013) showed, this approach is very error-prone in the context of a frequentist hypothesis test using only random intercepts.

Chapter 11.
General Discussion

To put it another way, the answer to the question, "I mean, what exact buttons do I have to hit?" *is that there is no such button.*

Andrew Gelman, 2015

Some thoughts outlined in this discussion have been published in:
Hesselmann, G., Moors, P. (2015). Definitely maybe: Can unconscious processes perform the same functions as conscious processes? Frontiers in Psychology, 6:584.

The central question we set out to address at the start of this dissertation was: *"What is the representation of a visual stimulus perceptually suppressed through continuous flash suppression, with a focus on perceptual organization?"*

Summary of the main findings

In Part One, we presented two studies in which the suppressive mechanisms of CFS itself were the topic of investigation. The motivation of these studies was to better understand the kind of suppression we are dealing with in CFS, because this has important implications for the interpretation of studies relying on the paradigm. Indeed, a large number of studies have made very general claims about what can and cannot be processed for stimuli perceptually suppressed through CFS. This stands in stark contrast with how little research has been devoted to unravelling the mechanisms behind the effective suppression induced by CFS. The studies in Part One of this dissertation are placed exactly in the context of a better understanding of the mechanisms behind CFS. The goal of these studies was to consider CFS in the context of binocular rivalry mechanisms in general and to explore whether CFS is just a stronger version of binocular rivalry or whether the dynamic and rhythmic nature of the CFS mask evokes mechanisms distinct from those known to play a role in regular binocular rivalry. In Chapter 2, we observed that the depth of suppression in CFS is feature-selective, as has been observed in regular binocular rivalry. That is, suppression depth was modulated by the feature overlap (i.e., motion speed) between CFS mask and suppressed stimulus. Furthermore, we showed that these results could not be explained by the predictions of a simple model relating the effectiveness of suppression to a reduction in neural adaptation to the CFS mask due to its transient nature. In Chapter 3, we studied the temporal dependency structure in time series of suppression times we obtained in several different b-CFS experiments. Akin to observations in regular binocular rivalry, we observed that these time series are not completely random and show robust serial correlations, providing a footprint of the underlying neural alternation process. Moreover, these serial correlations were most pronounced in situations in which the CFS mask was not swapped across trials, highlighting that the origin of these correlations was primarily monocular. Based on these results, we concluded that CFS and regular binocular rivalry rely on common rather than distinct mechanisms.

In Part Two, the focus of the chapters shifted to the "fate of the suppressed stimulus." That is, the shared focus of all these studies was to elucidate what the "representation" is of a stimulus perceptually suppressed through CFS. In Chapter 4, we showed that the surface that is induced in the well-known Kanizsa configuration was not the driving the differences observed in suppression durations. Rather, these differences that were observed across all different conditions could be related to differences in the orientation spectrum of the stimuli that were used. In Chapter 7, we observed that biological motion stimuli (point-light walkers) do not differentially enter awareness in the case of stimulus inversion, while motion coherence did influence suppression times (coherent walkers entered awareness faster than scrambled ones). In Chapter 8, half-face stimuli presented in the configuration as we commonly experience them broke suppression faster, but this familiarity effect did depend on face inversion. This result led us to conclude that the curvature relative to fixation (i.e., convex vs. concave) was mainly driving the difference in suppression times. In Chapters 9 and 10, we showed that seemingly high-level processing during CFS could either not be replicated (Chapter 9 on scene processing), or more basic aspects of the studied processing mechanism did not appear (Chapter 10 on word processing). Rather, in Chapter 9 we found that certain image properties of scene stimuli consistently broke suppression faster and in Chapter 10 we found that higher pixel density in the case of the word stimuli led to a difference in suppression times. Hence, both studies thus indicated no convincing evidence for high-level processing of stimuli perceptually suppressed through CFS. Furthermore, in Chapter 5, we obtained no evidence that a supraliminal auditory stimulus could influence processing of a perceptually suppressed visual stimulus, adding to the evidence that CFS impoverishes the representation of the suppressed stimulus to a great extent. Given the picture sketched from these findings, it was surprising to us, therefore, that we did obtain consistent evidence for launching events entering awareness faster compared to passing or pseudo-launch events (Chapter 6). Before putting this finding into the broader context of a current working hypothesis we have on the representational nature of the stimulus perceptually suppressed through CFS, we turn to a discussion of Chapters 2 and 3.

CFS – a stronger version of binocular rivalry after all?

When CFS was introduced by Tsuchiya and Koch (2005), they asked whether CFS was merely a stronger form of binocular rivalry. The authors concluded that this was not the case, but that CFS rather combines aspects of flash suppression and binocular rivalry (hence the name continuous flash suppression). Here, I will argue that the original arguments put forward to consider CFS as being different from binocular rivalry are insufficient. In addition, based on the evidence presented in Chapters 2 and 3, considerations from computational modeling of binocular rivalry, as well as pilot data, I will argue that the effectiveness of CFS mainly derives from a combination of three factors. First, the feature overlap between the mask and suppressed stimulus is a necessary, but not sufficient factor for effective suppression. Second, the transient induced by rhythmic updating of the CFS mask reduces neural adaptation to the mask. Third, the predictable, rhythmic nature by which the CFS mask is refreshed potentially acts as an entraining stimulus inducing cortical oscillations in the alpha frequency band leading to increased suppression depth because of gating by inhibition.

Considerations on CFS as a continuous form of flash suppression

In the original study, Tsuchiya and Koch (2005) documented that the temporal frequency at which the CFS mask is updated modulates the effectiveness of suppression. In this experiment, observers were presented with a sinusoidal grating stimulus in one eye perceptually suppressed by the CFS mask presented to the other eye. In a one-minute trial, observers had to track their percept (mask, grating, mix) while the temporal frequency of the CFS mask was varied across trials. The authors observed that the total and mean dominance duration of the CFS mask were modulated by the temporal frequency in an inverse U-shaped fashion. Peak durations were observed in the range of 3 to 12 Hz. The critical observation was that this pattern held for both total and mean dominance durations, which was not in line with Levelt's second proposition of binocular rivalry (Brascamp, Klink, & Levelt, 2015; Levelt, 1965). This proposition states that changing the strength of stimulus A (e.g., through manipulating temporal frequency of the CFS mask) will primarily affect the mean dominance durations of the other stimulus (here, the grating stimulus), leaving the mean dominance durations of stimulus A unaffected. Thus, mask temporal frequency did not

behave according to the predictions of "stimulus strength" according to Levelt's second proposition. Therefore, it cannot be conceptualized as such. This led the authors to argue that the relationship between mask temporal frequency and CFS effectiveness is better described akin to the effectiveness of flash suppression. That is, in flash suppression the effectiveness varies in function of the pre-adaptation period to the to be suppressed stimulus (i.e., the slower the pace at which the CFS mask refreshes, the longer the pre-adaptation period, and the more successful the next flash will be). In addition, Tsuchiya and Koch (2005) showed that the pattern of dominance durations could be described by a simple phenomenological model of binocular rivalry based on Levelt (1965), but only if a flash suppression component was added to the model (i.e., the probability of a successful continuation of CFS had to depend sigmoidally on the mask temporal frequency, with a higher probability of success for lower frequencies). This led the authors to argue that CFS is not simply a stronger form of binocular rivalry, but rather a continuous, accumulated form of flash suppression (Tsuchiya, Koch, Gilroy, & Blake, 2006). In a follow-up study, Tsuchiya et al. (2006) showed that suppression is also much deeper for CFS compared to binocular rivalry. Furthermore, the number of flashes during a trial modulates the depth of suppression such that they "synergistically summate to yield suppression equivalent to that measured using CFS (Tsuchiya et al., 2006, p. 1069)." I will argue that, when critically examined, these arguments run short of consistently explaining all findings. Furthermore, I think it is theoretically more coherent and parsimonious to explain CFS in terms of what we know of binocular rivalry and its underlying mechanisms.

First, the observations of Tsuchiya and Koch (2005) indeed do not follow Levelt's second proposition. It has recently been shown, however, that Levelt's second proposition only holds for a limited contrast range (Brascamp, van Ee, Noest, Jacobs, & van den Berg, 2006). When the suppressed grating stimulus is presented at maximal contrast, the proposition holds. When it is presented at moderate contrast, however, as is regularly the case in CFS, varying suppression strength *also* influences the mean dominance duration of the dominant stimulus (in this case the CFS mask). Thus, the observations of Tsuchiya and Koch (2005) are actually in line with other studies on binocular rivalry (Brascamp et al., 2006), yet the reason they did not follow Levelt's second proposition is presumably caused by the regime in which CFS operated during the experiment. Second, the flash suppression

component that was added to the phenomenological model to explain the behavioral results seems theoretically inconsistent. That is, in regular flash suppression, the canonical observation is that the longer the pre-adaptation period of the to be suppressed stimulus, the higher the probability this stimulus will be suppressed effectively by the stimulus presented to the other eye. In their model however, Tsuchiya and Koch (2005) implemented the flash suppression component such that the lower the mask frequency (i.e., longer adaptation to the mask), the more successful the mask refresh will be in maintaining the current percept. However, with respect to the relationship between mask frequency and effectiveness of suppression, their results showed an inverted U-shape rather than a sigmoidal pattern. Although their implementation of the model including a flash suppression component outperformed the simpler model, it seems theoretically untenable. Third, in a follow-up study, Tsuchiya et al. (2006) varied the number of flashes and the stimulus onset asynchrony between the number of flashes and the onset of a contrast increment probe. They observed that contrast detection thresholds were comparable to those obtained in a first experiment measuring suppression depth using regular CFS when there were five flashes and the stimulus onset asynchrony is zero. Based on these results, the authors argued that consecutive flashes summate to achieve suppression depth comparable to regular CFS. Therefore, CFS can best be considered as a continuous version of flash suppression. However, this interpretation suffers from a confound with probe duration. That is, probes were presented for 500 milliseconds, while five flashes also constituted 500 milliseconds of transient visual input. Therefore, it is hardly surprising that the highest detection thresholds were obtained when these events fully overlapped. When the stimulus onset asynchronies slightly diverged (i.e., +- 100 ms), detection thresholds associated with five flashes dramatically dropped. A more precise interpretation of these results thus would be that suppression depth of CFS peaks when transients in the CFS mask completely overlap with the presentation of the probe stimulus.

Rather than interpreting CFS as a continuous form of flash suppression, it might make more sense to interpret CFS as a continuous form of flash *facilitation* (Brascamp, Knapen, Kanai, van Ee, & van den Berg, 2007). Indeed, flash facilitation refers to a situation opposite to flash suppression in which previewing a stimulus (pre-adapting) actually facilitates the dominance of the stimulus subsequently presented to that eye. In their study,

Brascamp et al. (2007) showed that flash facilitation and suppression occurred in function of the contrast and duration of the prior stimulus. Interestingly, when the prior stimulus duration was set at 100 milliseconds (akin to the 10 Hz regularly used in CFS), flash facilitation was observed for all but the lowest contrasts. In addition, the authors showed that both facilitation and suppression can be modeled using a computation model of binocular rivalry (Noest, van Ee, Nijs, & van Wezel, 2007), unifying these apparently opposing phenomena to the same underlying mechanisms.

Considerations on CFS as a stronger form of binocular rivalry

In the light of these arguments, I propose it is more reasonable to consider CFS as a form of binocular rivalry that, under particular circumstances, elicits remarkably effective suppression. Nearly all computational models of binocular rivalry include two components to account for the seemingly stochastic, oscillatory behavior of the perceptual states. That is, two pools of neurons representing the stimuli presented to both eyes mutually inhibit each other, and a self-adaptation component is included for gradual decay of the stimulus strength of the current dominant percept (Noest et al., 2007; Wilson, 2007). Because such a circuitry is deterministic, white noise is often added to generate realistic dynamics. Several predictions follow when CFS is considered as a form of binocular rivalry. First, it should be possible to model the temporal dynamics of CFS using a computation model of binocular rivalry which relies on cross-inhibition and self-adaptation. Indeed, Shimaoka and Kaneko (2011) showed that the data reported in Tsuchiya and Koch (2005) and Tsuchiya et al. (2006) could be modeled with the model proposed in Wilson (2007) – with minimal extensions to the model. Second, feature-selective depth of suppression has often been observed in binocular rivalry (Alais & Melcher, 2007; Stuit, Cass, Paffen, & Alais, 2009). Similarly, as we reported in Chapter 2, and other studies have reported elsewhere, feature overlap between CFS mask and suppressed stimulus yields deeper suppression than when the CFS mask and suppressed stimulus are dissimilar (Hong & Blake, 2009; Maehara, Huang, & Hess, 2009; Moors, Wagemans, & de-Wit, 2014; Yang & Blake, 2012). Third, one would predict to observe serial dependence in dominance (or suppression) durations, as has been observed in studies on binocular rivalry and other bi-stability phenomena (Mamassian & Goutcher, 2005; Pastukhov & Braun, 2011; van Ee, 2009). Indeed, in Chapter 3 we reported that time series

obtained in several b-CFS studies show serial dependence, the size of which was comparable to previous studies. Furthermore, this serial dependence was predominantly monocular, in line with the monocular nature of binocular rivalry suppression that has frequently been observed (Blake, 1989; Blake & Logothetis, 2002; Tong, Meng, & Blake, 2006).

If CFS relies on similar circuits as binocular rivalry, how is such effective and deep suppression achieved? First, it is important to highlight that "shared stimulus complexity" or "feature overlap" is particularly important to achieve stable suppression. Indeed, when the suppressed stimulus and CFS mask do not "share" any neural resources, it is highly likely that the transient nature of the CFS mask will not achieve any deeper suppression at all. This is evident from our results in Chapter 2, for example, in which we observed that some motion speeds achieved similar suppression as regular CFS. Furthermore, as Figure 11.1 shows, researchers regularly adapt the properties of their CFS mask to match the suppressed stimulus. Indeed, informal discussions at conferences immediately made me realize that this was a very important factor for effective CFS suppression.

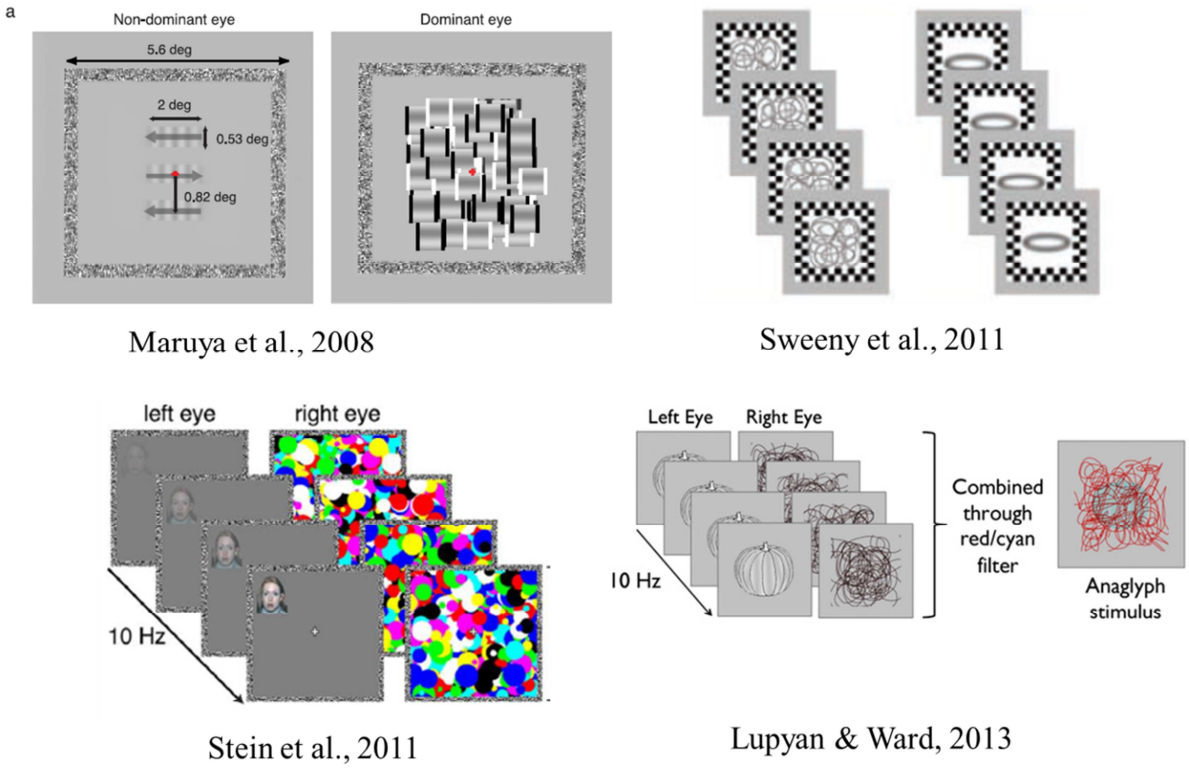


Figure 11.1 An illustration of different implementations of CFS. These studies are a subset of examples in which the CFS mask was adapted to include more features of the suppressed stimulus.

Secondly, one of the most straightforward explanations of the effectiveness of CFS – once stable suppression is achieved – is that neural adaptation to the mask is prohibited due to the continuous changes of the CFS mask. Indeed, in computational models of binocular rivalry perceptual switches are initiated when the activity of the dominant stimulus is no longer strong enough (due to self-adaptation) to inhibit the activity of the suppressed stimulus, resulting in a perceptual switch. If the visual input is continuously updated, however, this reduces adaptation to the mask, therefore prolonging the dominance durations substantially. A straightforward prediction derived from this explanation would be that CFS masks composed of repeating patterns are less effective compared to regular CFS because they allow for more adaptation to the CFS mask. This is exactly what we tested in a pilot experiment. The experiment was similar to the one reported in Tsuchiya and Koch (2005) where the relationship between mask temporal frequency and dominance durations was assessed. In our experiment, observers were presented with a sinusoidal grating stimulus in one eye and a CFS mask in the other. The CFS mask either consisted of two, four, eight, or no sequentially alternating patterns (see Figure 11.2). Observers had to continuously indicate their percept for one-minute trials (grating, mix, or mask). Six observers each completed 16 trials in total (4 trials in each condition).

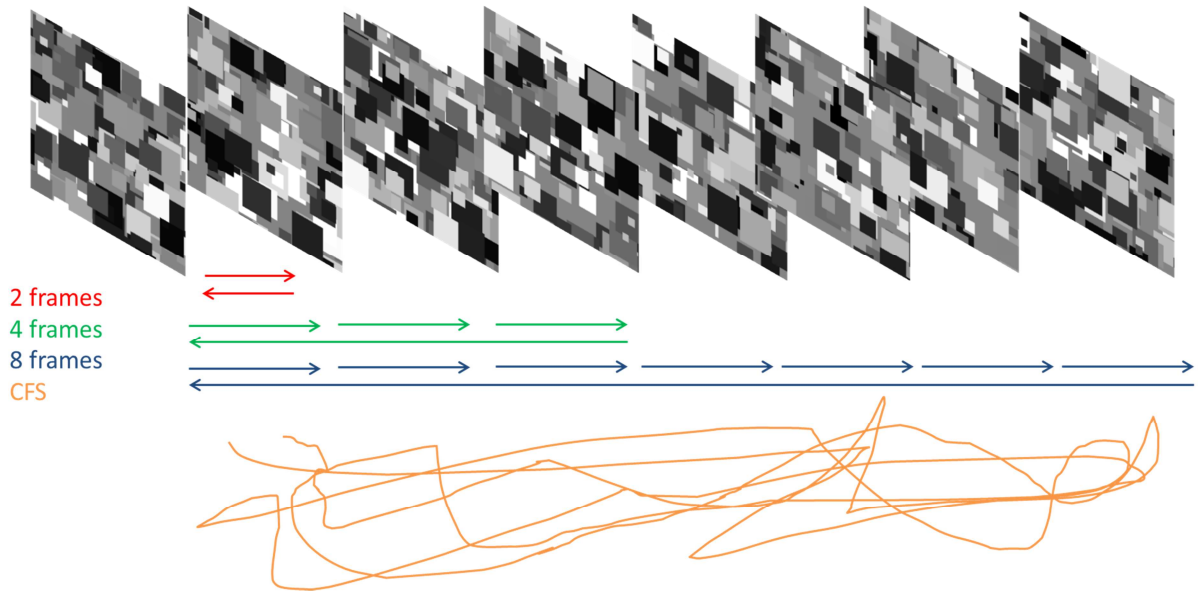


Figure 11.2. Illustration of the different conditions used in the experiment. The 2 frames condition consisted of two alternating masks. In the 4 frames condition four different masks were repeated in the same sequence, as was the case for the 8 frames condition. In the CFS condition, masks alternated randomly.

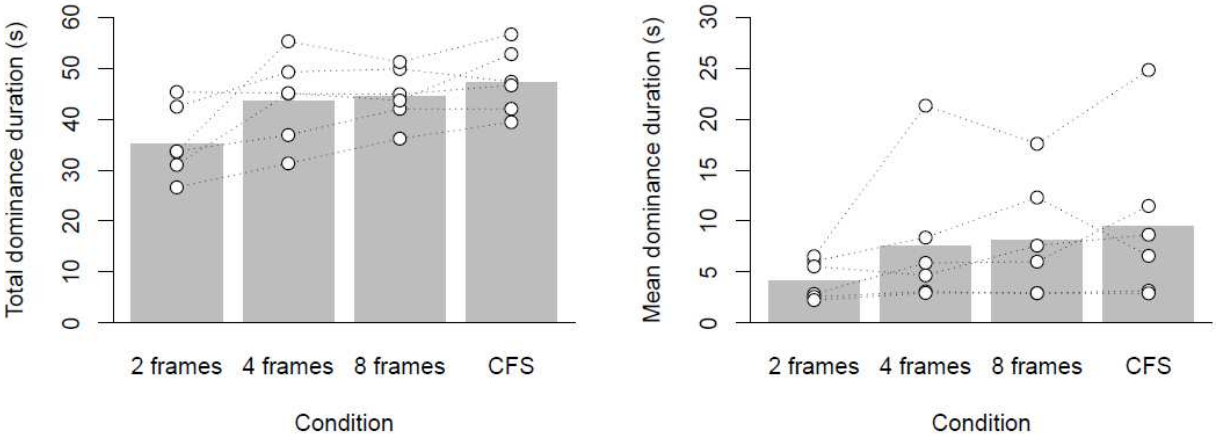


Figure 11.3. Results of pilot experiment on the effect of the number of unique frames in a CFS mask on dominance durations. (left) Total dominance durations of the CFS mask (in seconds) in function of experimental condition. (right) Mean dominance durations of the CFS mask (in seconds) in function of experimental condition. The number of frames indicates of how many frames the CFS mask was composed. 2 frames implies that the CFS mask was switching back and forth between 2 different patterns.

Figure 11.3 depicts the total and mean dominance durations of the CFS mask for each of the experimental conditions. A Bayes Factor analysis of the data revealed an effect of condition for both the total (BF > 100 compared to a model containing only a random effect of participants) as well as the mean dominance durations (BF = 7). This effect was in the direction predicted by an account based on reduced neural adaptation to the CFS mask. That is, total and mean dominance durations were shorter when the mask was composed of fewer frames. Interestingly (and this came to our attention after we conducted these pilot experiments), Tsuchiya (2005) reported on a similar unpublished experiment in his dissertation. In line with our observations reported here, he also reported an increase of both total and mean dominance duration of the CFS mask in function of the number of different frames of which the CFS mask is composed (i.e., as they increase, total and mean dominance durations of the CFS mask increase). Thus, based on the results of these two (unpublished) experiments, it seems reasonable to consider limited neural adaptation to the mask as an important factor contributing to the effective perceptual suppression induced by CFS.

On a more speculative note, a third factor that might be involved in the robust suppression induced by CFS is the rhythmic temporal structure by which the CFS mask is updated, and in particular the temporal frequency that is commonly used in CFS. Indeed, the canonical temporal frequency used in CFS studies is 10 Hz, which lies midway in the alpha frequency band (8-12 Hz) that can be measured using electro- or magnetoencephalography (EEG or MEG). A recent series of studies has provided evidence that the phase of the ongoing alpha rhythm plays an important role in stimulus detectability (Busch, Dubois, & VanRullen, 2009; Mathewson et al., 2012; Mathewson, Gratton, Fabiani, Beck, & Ro, 2009; Romei, Gross, & Thut, 2010; van Dijk, Schoffelen, Oostenveld, & Jensen, 2008). Furthermore, it has been shown that it is possible to entrain oscillatory activity in the alpha band by presenting rhythmic visual stimuli or by applying rhythmic stimulation through transcranial magnetic stimulation (TMS) and that this significantly influences stimulus detectability at threshold, in metacontrast masking, and in the attentional blink (Mathewson et al., 2012; Mathewson, Fabiani, Gratton, Beck, & Lleras, 2010; Romei et al., 2010; Spaak, de Lange, & Jensen, 2014). Based on this evidence, some authors have proposed that alpha rhythms act as a gating mechanism through pulsed inhibition. That is, depending on its phase, the alpha rhythm switches rapidly between phases of cortical inhibition and excitability, where the latter are time windows in which visual input can reach perceptual awareness (Jensen & Mazaheri, 2010; Mathewson et al., 2011). Thus, based on this account, the CFS mask could be considered a stimulus that entrains an alpha rhythm at the occipital part of the cortex, periodically inhibiting the stimulus presented to the other eye. The hypothesized involvement of alpha rhythms in the effectiveness of CFS could be tested in two different ways. First, one could consider the relationship between peak alpha frequency – a stable inter-individual trait (Angelakis, Lubar, Stathopoulou, & Kounios, 2004) – and the mask temporal frequency that yields the deepest suppression for each individual. If these are correlated, this might imply that an individually determined optimal temporal frequency at which the CFS mask is refreshed taps into the "natural alpha rhythm" of that individual. Second, if entrainment of alpha rhythms is genuinely mediating the effectiveness of CFS, it should be possible to pick up the periodic changes in suppression depth in function of the current phase of the alpha rhythm. We have attempted to test this latter prediction by measuring suppression depth of CFS across time. That is, we measured contrast detection

thresholds for a sinusoidal grating stimulus at various time points during a one-second trial, time points which were supposedly located at peaks and troughs of the ongoing alpha rhythm. Due to the short cycle of these rhythms however (i.e., 100 milliseconds), we had to present our target stimuli through very brief flashes. Recently, it has been shown that CFS is much less effective for abrupt, transiently presented targets (Kaunitz, Fracasso, Skujevskis, & Melcher, 2014). Due to the abrupt nature of our stimulus presentation, it proved very difficult to obtain any consistent results and this line of research was therefore no longer continued.

An alternative account of the effectiveness of CFS based on the broader framework of predictive coding

Before turning to a discussion of the results we presented in Chapters 4 to 10, we consider an alternative explanation of the effectiveness of CFS, relying on the predictive coding framework, which has recently gained considerable popularity (Friston, 2005; Mumford, 1992; Rao & Ballard, 1999).

Hohwy, Roepstorff, and Friston (2008) start from the idea that the brain is an inference machine (akin to Gregory, 1998; von Helmholtz, 1860). Perception can thus be conceptualized as a type of unconscious inference which can be formalized through hierarchical Bayesian inference relying on generative models with predictive coding or error minimization. In this view, perceptual content is determined by the hypothesis that generates the best predictions compared to the sensory input. The (visual) system matches bottom-up input with top-down predictions such that only the differences between them remain as a bottom-up signal – the prediction error. This notion of prediction error minimization is crucial to explain binocular rivalry (and perceptual bi-stability more generally) from a predictive coding framework. Indeed, the top-down predictions pertain to the dominant stimulus only, such that the bottom-up signal elicited by this stimulus is gradually explained away, yet the error signal associated with the suppressed stimulus remains, eventually causing the perceptual switch to this stimulus. Considering CFS within this context paves the way for an explanation of its effectiveness in terms of a general lack of reduction of prediction errors associated with the CFS mask (due to its continuously changing content) consequently prolonging dominance durations. Here we outline an experiment that aims to put this explanation of the effectiveness of CFS based on predictive

coding to the test by independently manipulating the spatial and temporal predictability of the CFS mask. Indeed, introducing predictability should make it easier to minimize prediction error and consequently decrease suppression strength. Importantly, predictability should be decoupled from adaptation, which we know influences the effectiveness of CFS (supra). In a potential experiment to test this hypothesis, one could measure suppression strength of CFS by determining contrast detection thresholds of a suppressed target stimulus in four different conditions (by crossing spatial and temporal predictability). The spatially unpredictable/temporally predictable condition is equivalent to how CFS is regularly implemented. That is, the mask content is updated at regularly spaced intervals (i.e., every 100 ms). In the spatially unpredictable/temporally unpredictable condition, the time points at which the mask will be updated will be randomized with the constraint that the mask has to refresh 10 times per second, yet at irregularly spaced intervals. In the spatially predictable conditions, the CFS mask alternates between a mask that contains squares and one that contains circles, yet the positions of the individual elements are still randomized at each refresh of the mask. The experimental procedure would be similar to Yang and Blake (2012), in which oriented Gabor patches were presented on each trial at a contrast level determined by an adaptive procedure and participants had to indicate the orientation of the Gabor patch on each trial. Instead of using trials in which the CFS mask and target stimulus are presented concurrently for a period of about a second, one could use continuous presentation of the CFS mask and present targets at regularly spaced intervals during this trial. The participants' task would be to indicate the location of the presented target. In this way, the target contrast can be adaptively increased when the targets go undetected and decreased again whenever they are detected. The reason why one would opt for continuous presentation of the CFS mask is that the spatiotemporal structure might need some time to be extracted and have its influence on suppression.

The predictions for this experiment are most clear for the spatial predictability. That is, if predictability of the spatial content matters for the effectiveness of CFS, suppression strength will be reduced in the predictable condition. For the temporal predictability, it is more difficult to derive a clear-cut prediction. That is, the visual system could "prefer" bottom-up signals in a temporally predictable fashion and consequently suppression strength might be highest for temporally predictable conditions. In contrast, if reduction of

prediction error works in a similar vein in the temporal domain, suppression strength is predicted to be highest in the temporally unpredictable condition. Nevertheless, irrespective of the direction of the effects, the results will shed light on how spatiotemporal predictability influences the dynamics of CFS. Furthermore, it would address the viability of the predictive coding framework as an explanatory model of binocular rivalry and CFS.

In the following section, we turn to a discussion on high-level processing during (b-)CFS, which is inspired by and further extends the ideas formulated in Hesselmann and Moors (2015).

Why high-level processing in b-CFS is theoretically incoherent

Since its introduction about ten years ago, CFS has been picked up relatively quickly as a potent technique to study unconscious processing of (visual) stimuli. Indeed, CFS has been described as "… a more optimal technique for examining preconscious processing mechanisms [compared to binocular rivalry]" (Yang, Zald, & Blake, 2007, p. 882) up to even "a cutting edge masking technique that allows subliminal presentations that last seconds. CFS is a game changer in the study of the unconscious…" (Sklar et al., 2012, p. 19614). The studies we presented in Chapters 4 to 10 inevitably provide a more nuanced, and perhaps slightly more realistic picture of the expectations one can have on the potential of CFS as a tool to study processing of perceptually suppressed stimuli. That is, the main picture that can be sketched based on our results is that the representation of a perceptually suppressed stimulus (measured through suppression times in a b-CFS paradigm) is a fairly impoverished and fractionated rather than integrated one, limited to very basic features such as orientation and spatial frequency (e.g., Chapter 4), and perhaps some more complex shape attributes such as convexity or complex image characteristics such as spatial coherence (e.g., Chapters 8 and 9).

In line with these observations, I would like to argue that the initial (and for some researchers, continued) enthusiasm for the potential of CFS to explore uncharted territories in consciousness research is most likely premature and farfetched. The principal reason for this argument derives from the fact that CFS highly likely relies on mechanisms similar to those observed in binocular rivalry. In this light, the problem with nearly all studies that embark on finding high-level unconscious processing during CFS ignore the representation

of the stimulus while it is rendered invisible. Indeed, any paradigm that perceptually suppresses a visual stimulus does so by interfering with the processing of the stimulus in some way (Fogelson, Kohler, Miller, Granger, & Tse, 2014). If not, the stimulus would always be visible to the observer. CFS is closely related to binocular rivalry, of which it is known that the activity related to the perceptually suppressed stimulus is mainly confined to early visual areas (Alais, 2012; Blake & Logothetis, 2002; Logothetis, 1998; Tong et al., 2006). Indeed, rather limited cognitive processing has been observed during binocular rivalry (Blake, 1988; Cave, Blake, & McNamara, 1998; Kang, Blake, & Woodman, 2011; Zimba & Blake, 1983). Therefore, it seems reasonable to adopt the default stance *not* to expect much high-level unconscious processing during CFS (Breitmeyer, 2015). Indeed, recent neuroimaging data suggests that the presence of CFS masks dramatically reduces neural activity related to the suppressed stimulus already in early visual cortex (Yuval-Greenberg & Heeger, 2013). Thus, the representation of the suppressed stimulus is expected to be rather limited to a loose collection of elemental features that are presumably coded in these early visual areas, despite the fact that the stimulus is presented unbeknownst to the observer for extended periods of time (Gayet, Van Der Stigchel, & Paffen, 2014).

Given that most studies on binocular rivalry were already conducted prior to the introduction of CFS, why have so many researchers embarked on unraveling high-level unconscious processing using CFS? I think the fact that CFS allows for such robust perceptual suppression has led many researchers to think along the following lines: "Because CFS allows to present a stimulus unconsciously in the order of seconds, this might allow for *more elaborate* processing compared to other blinding paradigms such as binocular rivalry or visual masking." At least, I have heard this argument many times when fellow researchers were presenting their results at a conference. However, this type of reasoning exactly ignores the representation of the stimulus during perceptual suppression. It seems very unlikely that a fractionated stimulus, reduced to its basic features will evolve into an integrated one, if the processing mechanisms that are usually involved for integrating features of a stimulus never come into play during CFS. Given that one of the first studies using b-CFS then shows that upright faces break suppression faster than inverted ones, as well as that Chinese words break suppression faster for Chinese speakers compared to Hebrew speakers and vice versa (Jiang, Costello, & He, 2007), it became tempting to push the limits of unconscious visual

processing further and further, until we can read and do arithmetic unconsciously (Karpinski, Yale, & Briggs, 2016; Sklar et al., 2012).

When I outline these arguments to fellow researchers at conferences, I often get the following response: *"But why have so many researchers found evidence for so-called high-level processing of perceptually suppressed stimuli in b-CFS experiments? Why should I believe you? Why can I not rely on the studies that have been published in international peer-reviewed highly esteemed journals?"*[6] I usually try to respond to this question in a diplomatic way but here, I will put it bluntly: I think the majority of positive findings on high-level processing during b-CFS are Type I errors, potentially due to intentionally (Simmons, Nelson, & Simonsohn, 2011) or unintentionally (Gelman & Loken, 2013) undisclosed flexibility in data analysis. The problem is that, with the best possible intentions, I cannot provide a proof for this argument. None of these published studies have provided the data along with the paper, let alone pre-registered the experiments that are reported. There is no way for me to even verify the published results, without having to contact the original authors. Second, for all studies that do not belong to this category and of which the findings prove to be replicable, I think a simpler explanation more than likely will be found. Our studies on the Kanizsa stimulus (Chapter 4) and the half-face stimuli (Chapter 8) are excellent cases in point. Indeed, in both studies we obtained results that could be interpreted as evidence for high-level processing during CFS. In both cases, however, the critical difference between conditions could be explained by lower-level aspects of the stimuli. Last, I think the CFS literature severely suffers from a file drawer problem (Rosenthal, 1979). Conferences and workshops have proven an invaluable source of information on this topic, and I know of several (unreported) studies that failed to replicate when tested by other labs. In addition, I also learned about studies starting off with a novel research question but which eventually were not published because they failed to reveal a difference between the experimental conditions – even though they revealed really interesting and important information on stimulus processing during CFS. Although this is a general problem in psychological science, I think CFS studies are a particularly important example of this problem because they can substantially influence contemporary theories on the scope and limits of unconscious visual processing. In this respect, we have tried to provide some counterweight to this issue by reporting all our completed studies irrespective

[6] Ok, I admit I made up that last question

of their outcome. In fact, the type of discussion outlined here could never have arisen should we have adopted a different strategy throughout our research. Personally, I think consciousness research and research on unconscious visual processing in particular would benefit greatly from an attitude change towards embracing null findings showing that this or that paradigm does not allow for this or that type of processing. Such an attitude change is tightly linked to adopting the appropriate statistical methods that allow for such conclusions to be drawn on statistical grounds (e.g., Bayesian methods). Although there is still a long and bumpy road ahead of us, I do think the tide is already turning for consciousness research, thanks to other advocates of this stance (Dienes, 2015, 2016).

CFS – a tool to probe the default processing mode of early visual cortex or the low-level statistics of the environment we are sensitive to

It seemed pretty straightforward to devise an argument for why high-level processing should not be observed in b-CFS (or in any kind of CFS study). It proves to be much more difficult however to outline a coherent hypothesis about what exactly is driving the differences in suppression times observed in b-CFS studies or the differences in reaction times in priming studies relying on CFS. I preface that, given my beliefs on high-level processing in b-CFS studies, the discussion outlined here will necessarily be a biased one. Some published findings will fit the discussion and others will not.

The general philosophy behind b-CFS is that breakthrough or suppression time reflects the strength of the representation of the perceptually suppressed visual stimulus. That is, as in regular binocular rivalry, "stimulus strength" is predicted to influence suppression durations such that stronger stimulus representations break CFS faster than weaker stimuli. We are stuck however at this concept called "stimulus strength". Indeed, it is not a well-defined construct and different researchers might mean different things when using the term. Therefore, it would be very useful to attempt to delineate this concept in the context of perceptually suppressed stimuli, because it can guide future hypothesis generation.

Our current hypothesis is that b-CFS reveals the default (or current) sensitivity of early visual cortex to input statistics we have been exposed to throughout our life (e.g., color,

orientation, spatial frequency, etc.), which in turn has shaped the neural representation of these statistical features as well as the strength of their connectivity pattern (Field, 1987; Simoncelli & Olshausen, 2001). Here, it is critical to stress that these sensitivities pertain only to the level at which the stimulus is still represented during interocular suppression. This inevitably implies that some statistical regularities in the environment that are obvious to us might not be revealed through b-CFS, because the representation of the suppressed stimulus might not yet be sufficiently differentiated at the level of early visual cortex. What does this hypothesis imply and, importantly, what does it not imply?

First, as already highlighted, stimuli that require some form of contextual integration relying on iterative feedforward-feedback loops such as the Kanizsa stimulus (Chapter 4) or the biological motion stimulus (Chapter 7) are not processed as such during CFS. Indeed, in line with our observations, other studies have shown that three-dimensional cast shadows or implied motion are not processed during CFS (Faivre & Koch, 2014; Khuu, Gordon, Balcomb, & Kim, 2014), nor are faces processed holistically (Axelrod & Rees, 2014). Second, simple stimulus features should have a processing benefit during CFS. For example, a radial bias for orientation and motion direction has been observed in suppression times (Hong, 2015), as well as a preference for collinear contours (Li & Li, 2015). Furthermore, suppressed collinear flankers have also been observed to still influence detection of a visible target (Hayashi & Murakami, 2015). Last, for dynamic stimuli, motion coherence also seems to systematically influence suppression times (Chung & Khuu, 2014; Kaunitz, Fracasso, Lingnau, & Melcher, 2013).

It is important to stress that this view does not exclude the possibility of modulatory or top-down effects on suppression times. Indeed, the contents and load of visual working memory have been shown to influence suppression times (De Loof, Poppe, Cleeremans, Gevers, & Van Opstal, 2015; Gayet, Paffen, & Van der Stigchel, 2013; Pan, Lin, Zhao, & Soto, 2013), as well as expectations (Pinto, van Gaal, de Lange, Lamme, & Seth, 2015; Stein & Peelen, 2015), and the expertise of the observer (Stein, Reeder, & Peelen, 2015). Furthermore, and contrary to our findings in Chapter 5, stimuli presented together with congruent auditory stimuli have been shown to enter awareness faster (Cox & Hong, 2015; Tan & Yeh, 2015). The important point here is that these modulatory or top-down influences play out at the representational level of the suppressed stimulus. Indeed, in an elegant study, Lupyan

and Ward (2013) showed that presenting an auditory cue that matched a perceptually suppressed object (e.g., "kangaroo") speeded up the breakthrough of this stimulus compared to incongruent verbal labels. Critically, the authors showed that this facilitatory effect occurred at the perceptual rather than semantic level, implying that effects that "behave like" semantic processing are actually better explained by processing at the early, perceptual level.

A good case in point here is the recent study of Gelbard-Sagiv, Faivre, Mudrik, and Koch (2016). They showed that repetition priming for famous faces *only* occurred when observers were aware of the color or location of the face (i.e., "low-level awareness") without being aware of its identity. On trials in which suppression was complete, no repetition priming was observed. So far, so good. The authors concluded, however, that low-level awareness of the perceptually suppressed face stimulus accompanies "high-level processing" of it. It should be stressed that there is absolutely *no* principled reason to interpret these results as providing evidence for *identity* processing of the suppressed face stimulus. The *only* thing that is being shown here is that repeating an invisibly presented face stimulus yields faster responses to the repeated visible face stimulus. This is not a strong test for so-called high-level processing of invisible stimuli. Any perceptual feature the visual system is sensitive to during CFS can yield faster responses. A beautiful illustration of this point are the studies by Almeida and colleagues (Almeida et al., 2013; Almeida, Mahon, & Caramazza, 2010; Almeida, Mahon, Nakayama, & Caramazza, 2008). Based on the reasoning that dorsal stream processing would be preserved during CFS (Fang & He, 2005; Ludwig & Hesselmann, 2015), the authors obtained evidence that perceptually suppressed tools (a supposedly distinct category of objects to which the dorsal stream would be primarily sensitive to) could prime other tools, whereas animals (supposedly processed by the ventral stream, which is blocked by CFS) failed to prime other animals. In an attempt to pinpoint what was actually happening during tool priming, Sakuraba, Sakai, Yamanaka, Yokosawa, and Hirayama (2012) showed that stimulus elongation was the critical factor, independent of whether these stimuli belonged to the tool category or not. The importance of this study should not be underestimated. It shows that it is critical to exclude potential alternative explanations before you advance to an interpretation based on high-level stimulus processing during CFS. Although this issue applies to any experimental study, it again seems particularly important in the context of inferring the scope and limits of unconscious visual

processing. I personally think a particularly strong test of the extent to which perceptually suppressed stimuli are processed is to use a paradigm in which performance on a visible stimulus is measured while invisible stimuli are presented concurrently which, under normal visible conditions, elicit an influence on the visible target stimulus. A good example of a study that used this strategy examined whether flanker interference (i.e., reduced task performance when flanking stimuli are incongruent with respect to the target stimulus on a task-relevant stimulus dimension) would still be elicited by invisible flanker stimuli (Wu et al., 2015). The authors presented arrows as flanker stimuli, either visible or suppressed. Participants had to perform a task on a central visible arrow (i.e., indicate its pointing direction). The results revealed that flanker interference was observed in the fully visible trials, whereas *no* flanker interference was observed on the suppressed trials. This indicates that the representation of the flanking arrows was not sufficiently "arrow-like" to initiate a conflict with the central, visible arrow. We ourselves have conducted a similar study aimed at unraveling whether more basic contextual modulation at the perceptual level would occur when the context of a stimulus was suppressed. To do so, we used a grating stimulus that is perceived as if it is presented through an aperture. This renders the perceived motion direction of the stimulus ambiguous in theory, but in practice observers frequently perceive motion in the direction perpendicular to the orientation of the grating (illustrated in Figure 11.4A). Adding a context to this stimulus can bias the perceived motion direction, as illustrated in Figure 11.4B. The local motion trajectory within the aperture remains exactly the same, yet due to the configuration, a rectangular grating is perceived to move to the left behind an occluder. We presented the aperture stimulus either with or without context, as well as either with or without a CFS mask, and asked observers to indicate the motion direction they perceived for the inner stimulus (trial sequence depicted in Figure 11.4C). We presented stimuli that were moving in either of four directions (left, right, up, down). Each observer (eight in total) completed 40 trials per condition (160 in total). We summarize the data in Figure 11.4D as the proportion of perceived motion in "cardinal" directions (i.e., left, right, up, down). As is obvious from the data, adding the context to the stimulus strongly biased the perceived motion direction in all but one observer. In all other conditions, however, the perceived motion direction substantially dropped to baseline levels of perceiving "oblique" directions (down left, down right, up left, up right). These preliminary

results indicate that perceptually suppressing a stimulus eliminates the potential to integrate a stimulus with its contextual surround. For this type of processing, presenting the context stimulus under suppression is similar to not presenting it at all.

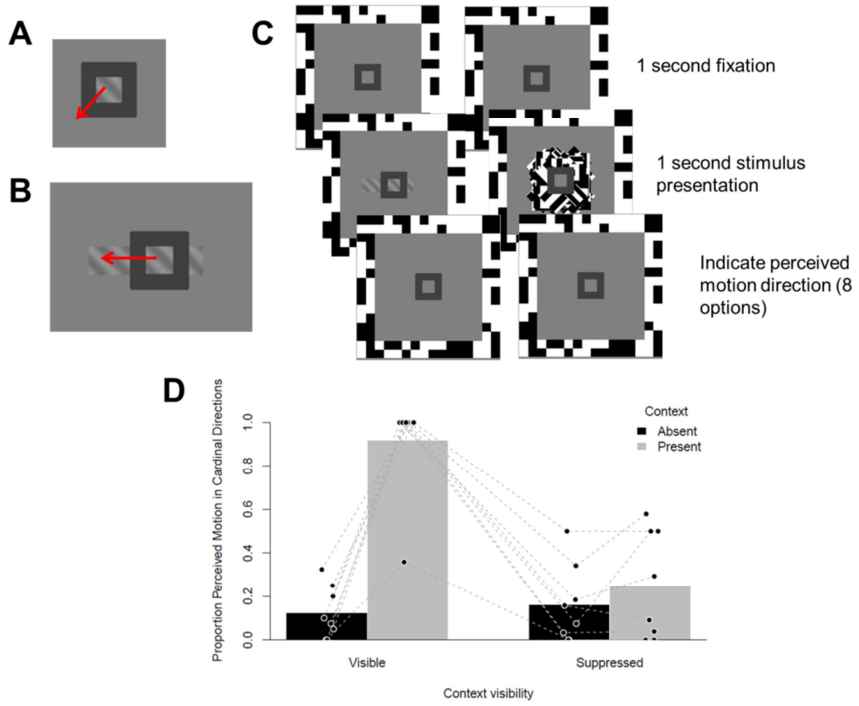


Figure 11.4. Preliminary experiment on contextual integration between visible and invisible stimuli. (A). A translating grating stimulus that appears to move behind an aperture. The perceived motion direction that is most frequently reported is the one perpendicular to grating orientation. (B) Adding a context to the grating stimulus biases the perceived motion direction towards the left direction, despite the same physical input in the central region as in stimulus A. (C) Trial sequence used in the experiment. The grating stimulus was presented for 1 second in the non-dominant eye (with or without a context, and with or without a CFS mask in the other eye). Observers had to indicate the perceived motion direction (one of eight possible responses). (D) Mean proportion of perceived motion in cardinal directions (n = 8).

Another prediction that could be derived from framing differences in suppression times as reflecting the sensitivity to "primitive" stimulus features is that, due to the plasticity of the visual system, it should be possible to change representational strength through perceptual learning or through changing stimulus relevance by means of conditioning

paradigms. Indeed, it has been shown that stimuli break through faster and more frequently when participants repeatedly perform the same task across different blocks, sessions, or even days (Carmel, 2015; Ludwig, Sterzer, Kathmann, Franz, & Hesselmann, 2013; Mastropasqua, Tse, & Turatto, 2015). Similarly, coupling invisible stimuli with rewards changes their suppression depth (Seitz, Kim, & Watanabe, 2009) or presenting electric shocks together with perceptually suppressed stimuli influences the electrodermal response to those stimuli (Lipp, Kempnich, Jee, & Arnold, 2014; Raio, Carmel, Carrasco, & Phelps, 2012). Furthermore, pairing an initially neutral and visible grating stimulus with electric shocks also influences suppression times such that grating stimuli associated with shocks break suppression faster (Gayet, Paffen, Belopolsky, Theeuwes, & Van der Stigchel, 2016). Interestingly, stimulus-reward coupling for invisible stimuli is monocular (Seitz et al., 2009), whereas triggering the relationship prior to suppression speeds up breakthroughs in both eyes (Gayet et al., 2016). Furthermore, Hedger, Adams, and Garner (2015a) showed that threatening stimuli did not elicit differential skin conductance responses compared to neutral stimuli when they were completely suppressed. Only when these stimuli broke suppression during a trial, a differential electrodermal response was observed. This provides an important insight as to how threatening or emotional stimuli are represented during suppression. Indeed, it has been debated whether the advantage of fearful faces breaking suppression faster than neutral faces has more to do with their spatial frequency content than with their emotional value (Gray, Adams, Hedger, Newton, & Garner, 2013; Hedger, Adams, & Garner, 2015b; Stein, Seymour, Hebart, & Sterzer, 2014; Yang et al., 2007). Vice versa, stimuli that do not elicit a threat response under normal conditions can be paired up such that they do under conditions of suppression. On a final note, two very recent studies showed that associating grating stimuli with the self or the other (i.e., by associating it with the word "you" or "other") did not influence suppression times of the gratings (Stein, Siebold, & van Zoest, 2016). Furthermore, associating faces with affective information similarly does not influence subsequent suppression times of these faces (Rabovsky, Stein, & Abdel Rahman, 2016). This seems to indicate that not any type of coupling between stimuli can influence processing of perceptually suppressed visual stimuli.

The last aspect of our results we need to consider in the light of this view are those presented in Chapter 6, on the perception of causality. In this chapter, we reported two

experiments of which the results indicated, to our surprise, that launch events consistently enter awareness faster than pass events or pseudo-launch events. If the framework laid out above is correct, this result can be interpreted in at least two ways. Either the visual system is already very sensitive to spatiotemporal contingencies at a very early level, or we still have not pinpointed the low-level confound that can explain our results. Related to this latter point, it might be interesting to consider the control event that was originally used in the causality adaptation study of Rolfs, Dambacher, and Cavanagh (2013). Here, the authors used a "slip" event in which a first disc approached a second stationary disc, passed over it, and stopping just in front of the second disc. Subsequently, the second disc would start moving. This event might provide an even more stringent control condition in which the low-level cues (i.e., motion discontinuities are now both on the same trajectory as the launch event) are nearly completely matched. If the suppression time advantage of the launch event survives this test, our results might unequivocally be interpreted as providing evidence for a sensitivity to causal events at very early stages of visual processing.

In sum, in this section we proposed that differences in suppression times (and other measures used in CFS experiments) reveal the representational strength of the suppressed stimulus at the level of early visual cortex reflecting the image statistics we have been exposed to throughout our life. In the final section of this General Discussion, we turn to the broader implications of having different blinding paradigms that yield different results for the same question.

A hierarchy of blinding paradigms: implications for the study of invisible stimuli

Throughout this dissertation, we have only relied on a single blinding paradigm to study the representation of perceptually suppressed visual stimuli. The main conclusion that could be derived from our results is that, for CFS, processing of invisible stimuli is highly likely limited to simple, visual features such as orientation, spatial frequency, motion coherence etc. and only limited, if any, spatiotemporal integration is taking place during CFS. Our conclusions are necessarily limited to CFS only. Therefore, a natural question to ask is how our results compare to those obtained with other suppression paradigms asking similar questions. As we already touched upon in Chapter 1, many other paradigms exist to render stimuli invisible and different paradigms have often yielded different results,

resulting in a so-called "functional hierarchy of unconscious visual processing" according to Breitmeyer (2015). Furthermore, a recent neuroimaging study showed that different blinding paradigms also yield different neural activation for the perceptually suppressed stimulus (Fogelson et al., 2014). Studies which have explicitly compared visual processing of invisible stimuli across different blinding paradigms have provided additional evidence for differential processing of invisible stimuli under different suppression conditions (Faivre, Berthet, & Kouider, 2012; Izatt, Dubois, Faivre, & Koch, 2014; Peremen & Lamy, 2014; Stein et al., 2014). For example, more elaborate processing is taking place under conditions of gaze-contingent crowding or visual masking compared to during CFS with respect to identity priming of famous faces (Izatt et al., 2014) or processing of an invisible arrow prime stimulus (Peremen & Lamy, 2014).

Although the results of these studies might not be all too surprising, what are we to conclude if different results on unconscious visual processing are obtained with different suppression paradigms? For one, it highlights that a study reporting a null finding yet relying on a single paradigm can never conclude that "processing of X depends on visual awareness". In the case of CFS studies researchers have all too often concluded this however, with respect to implied motion (Faivre & Koch, 2014), 3D cast shadows (Khuu et al., 2014), or the mere exposure effect (de Zilva, Vu, Newell, & Pearson, 2013). To the contrary, what these studies indicate is that, during interocular suppression, the representation of the stimulus is such that process or phenomenon X just cannot unfold.

Secondly, it renders the discussion on so-called unconscious high-level processing somewhat superfluous. Indeed, as long as we have a paradigm at our disposal that allows for sufficient processing of the stimulus whilst ensuring invisibility, one can in principle expect unconscious processing of anything. Take visual masking for example. Various studies have reported high-level processing of masked stimuli, such as semantic priming (Van den Bussche, Van den Noortgate, & Reynvoet, 2009), cognitive control (Lau & Passingham, 2007; van Gaal, Ridderinkhof, Scholte, & Lamme, 2010), or conflict adaptation (Desender, Van Lierde, & Van den Bussche, 2013; van Gaal, Lamme, & Ridderinkhof, 2010). In contrast, we failed to obtain evidence for semantic processing of word stimuli suppressed through CFS in Chapter 10. Does this invalidate the findings from the visual masking literature and imply that unconscious semantic processing cannot occur? To the contrary, it

only highlights the specificity of the results to the paradigm used to render visual stimuli invisible. Indeed, for visual masking it has been argued that the masked stimulus does not enter visual awareness because re-entrant activation from higher cortical areas is nearly absent, yet the feed-forward sweep of activation associated with the presentation of the masked stimulus is largely intact (Breitmeyer, 2008; Macknik & Livingstone, 1998). This difference in mechanism most likely explains why more high-level processing is observed in the visual masking literature compared to CFS.

What do our results and this discussion imply for the role of perceptual organization in the absence of visual awareness? Certainly, it does not exclude the possibility for perceptual organization in the absence of visual awareness. Indeed, using different paradigms, it has been shown that Gestalt grouping principles influence processing of an invisible stimulus. For example, Mitroff and Scholl (2005) have shown that connectedness, good continuation, proximity and common region all can influence processing of invisible stimuli during motion-induced blindness. Furthermore, it has recently been shown that masked Kanizsa configurations can have an influence on a subsequent shape discrimination task (Poscoliero, Marzi, & Girelli, 2013). Lastly, from the literature on visuospatial neglect, it is clear that Gestalt grouping cues have an influence on the severity of extinction (Conci et al., 2009; Driver & Mattingley, 1998; Mattingley, Davis, & Driver, 1997). The critical thing here is that perceptual suppression in these paradigms or neuropsychological conditions presumably entails mechanisms that are different from CFS and binocular rivalry.

The observations that studies on unconscious (visual) processing might reach different conclusions depending on the blinding paradigm that is used has led some to argue that unconscious visual processing is not a single entity, and that while all conscious visual processing is alike, each unconscious visual process is unconscious in its own way (Block, 2015). It seems to be the case that most researchers find this a satisfying stance since it is frequently acknowledged in recent papers that it is crucial to understand the similarities and differences between different suppression techniques to properly understand unconscious visual processing (Dubois & Faivre, 2014). Due to the abundance of different suppression paradigms, the literature is necessarily fragmented, but this has never been used to call the notion of unconscious visual processing per se into question. What do we actually mean with

"unconscious visual processing" when the answer depends on the paradigm used in the experiment? Is the overarching research question on the scope and limits of unconscious visual processing still an interesting one when we know up front that we will not be able to provide a unitary and coherent answer to the question? Might it not be better to drop the term unconscious visual processing all along? Or should we embrace the heterogeneity of these findings until a theory of consciousness unifies them?

Are there any alternative views from which we can study the similarities and differences between perceptually suppressed and visible stimuli? In my opinion, it might be worthwhile to consider the human visual system, and by extension the human observer, as (some)one that continuously seeks to interpret the proximal stimulus in order to act onto its environment. In some situations this "interpretative act" will yield perceptual suppression of the visible input, whereas small tweaks to the input dramatically change the percept to one in which the previously invisible stimulus is completely visible. For the case of visual masking, a recent paper by Herzog, Hermens, and Öğmen (2014) highlights that invisibility might sometimes be a goal the visual system achieves rather than an inherent limitation to the temporal precision of the visual system. Indeed, the authors provide several examples in which the effectiveness of visual masking is substantially affected when the masked and masking stimulus can be perceptually grouped (Hermens, Scharnowski, & Herzog, 2009; Herzog & Fahle, 2002). These grouping effects are also well-known in crowding (Manassi, Sayim, & Herzog, 2012, 2013; Saarela, Sayim, Westheimer, & Herzog, 2009; Saarela, Westheimer, & Herzog, 2010), flash-induced perceptual fading (Vergeer & van Lier, 2007), and binocular rivalry (de Weert, Snoeren, & Koning, 2005). Furthermore, substantial individual differences in the efficacy of visual masking (Albrecht, Klapötke, & Mattler, 2010), perceptual bi-stability (Carter & Pettigrew, 2003; van Loon et al., 2013), or continuous flash suppression (Yamashiro et al., 2014) indicate that perceptual suppression is not an all-or-none or off-the-shelf tool. Therefore, the way in which invisibility is achieved is presumably dependent on a complex interplay between inter-individual and stimulus-related factors which in itself might be a more interesting topic to study rather than whether we can solve invisible polynomials (Sklar & Hassin, 2015) or can extract the syntactic structure of invisible words (Hung & Hsieh, 2015).

Concluding remarks: a plea for combining blinding paradigms, respecting the absence of a difference, and increasing transparent research practices

I end this General Discussion with some considerations on my view on how studies on unconscious visual processing should proceed to be maximally informative. I should note that I myself have run short on some of these issues. In hindsight, you can always do better, and I am the first to acknowledge that.

First, if you genuinely want to study unconscious visual processing, I think it is of great importance to design experiments that rely on all sorts of different blinding paradigms. Even if these yield conflicting results, this provides crucial information on the level at which certain aspects of invisible stimuli can be processed. In the end, this might yield a more satisfying picture of the hierarchy of processing invisible stimuli. Second, you should include the most stringent control conditions imaginable. This might seem an obvious or even redundant consideration, but all too often I came across CFS studies for which I could think of control conditions that could yield an explanation of the data based on much simpler mechanisms. Third, if you fail to find an effect, do not start tinkering with the data, or with the design. If you tweak a sufficient number of times, I can guarantee you have a 100% chance to stumble upon something (something too good to be true, that is). Do not let the asymmetry of statistical inference bug you. And do not put it in your file drawer (really, don't). Think hard about whether it makes sense *not* to expect a difference. As I argued before, in the case of CFS studies it might actually make sense to predict the absence of a difference. I am sure in many other domains of psychology this is also true. Last, be as open as you can during your research and when you report it. There really is no good reason for sneaking that prediction you didn't make before the data were known into the introduction of that paper (not even if this makes a huge difference for the journal in which you can publish your results or if Reviewer 2 asks you). There really is no good reason for not publicly sharing your data and analysis code. People will understand if not everything is crystal clear in your set of experiments, they probably have experienced that too. Last, but maybe most importantly, as Richard Feynman put it so elegantly in 1974 : *"The first principle is that you must not fool yourself – and you are the easiest person to fool."*

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